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THE Psychological Review

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EDITED BY

JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Index*)
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*) AND
ARTHUR H. PIERCE, SMITH COLLEGE (*Bulletin*)

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The Psychological Review.

THE INFLUENCE OF TEMPERATURE AND THE ELECTRIC CURRENT ON THE SENSIBILITY OF THE SKIN.[1]

BY THOMAS VERNER MOORE,
Catholic University of America.

I. Introductory.

Within the last few years the progress in physical chemistry has given an insight into physiological processes which before were not even amenable to an exact investigation. On becoming familiar with this movement at Professor Loeb's laboratory at the University of California, it occurred to me that it might be useful to apply the physico-chemical methods to psychophysics and investigate from a new point of view the relation between the stimulus and the sensation.

It is at least possible that the physiological process of sensation may depend on the velocity of a definite chemical reaction. If so, it would in some way vary with the temperature in accordance with the van't Hoff-Arrhenius law. My first attempt to investigate this problem was made with sensations of taste. No definite results were obtained. It was, however, proved by Weber in 1847 that temperature does affect the sensation of taste and that there are limits of temperature beyond which taste is impossible.[2] Between these limits it should vary according to a definite law, but its investigation would be involved in difficulties. At Professor Loeb's suggestion I left the problem of taste for that of touch. I made some preliminary experiments on the touch threshold at Berkeley.

[1] From the psychological laboratory of the Catholic University of America.

[2] 'Ueber den Einfluss der Erwärmung und Erkaltung der Nerven auf ihr Leitungsvermögen' (Müller's), *Archiv für Anat. und Physiol. und wiss Med.*, 1847, 2d pt., pp. 342–356.

These resulted in showing that the touch threshold varies with the temperature of the skin. The work has been continued at the Catholic University of America in Washington, under the direction of Doctor Pace, to whom I am thankful for many suggestions and for constant assistance. Among others, I am especially indebted to my subjects, Mr. Bour (B.), Mr. Ferris (Fe.), Mr. Finnegan (F.), and Mr. Hoey (H.).

II. The Method of Varying the Temperature of the Skin.

To vary the temperature of the skin I placed over the area investigated — the upper portion of the inner side of the right forearm — a hot water bag filled with water of a known temperature. The temperature of the skin thus produced is not that of the water in the bag, nor will it be that of a thermometer between the arm and the bag, but either less or greater than that of the thermometer according as the bag is above or below the physiological zero. The thermometer approximates the arithmetical mean between the temperature of the bag and that of the arm. A thermometer with cylindrical bulb placed between two hot water bags varies from the arithmetical mean in opposite directions according as the hot or the cold bag is on top. The mean of two such readings is not far from the arithmetical mean of the temperature in the two hot water bags. It is always a little above — a phenomenon probably due to conduction currents which are faster in the warm than in the cold water. If the conductivity of the skin approximates that of rubber — and both are poor conductors — the temperature of the thermometer will approximate the arithmetical mean between the skin temperature and that of the bag. It can thus be calculated with a degree of accuracy that will be greater in the region of the physiological zero and will decrease on either side of that zero.

III. Influence of the Temperature of the Skin on the Spatial Threshold.

In investigating the 'spatial' threshold I used the usual bar compass, but to the customary points I attached two horse hairs of equal length. The diameters of the cross section of the top of each hair were .24 × .22 mm. measured by the microscope.

The bending power varies according to the strength of the stroke and the extent to which the hair is bent. The bending power of one was between 8.3 and 7.0 gr., that of the other between 8.7 and 8.0 gr. In giving the stimulus the attempt was made to acquire a stroke of constant pressure. The flexibility of the hairs makes this easier than when compasses with stiff arms are used.

The area measured was the upper portion of the inner forearm. Only the transverse threshold was measured. The arm was cooled or warmed by water contained in an ordinary hot water bag. This bag, of course, had to be raised from the arm in order to give the stimulus, but was immediately replaced. As the temperature of the bag differs from that of the room, there is some source of error due to the temporary removal of the bag.

Below are given the results for one subject. Under Δ is given the threshold distance between the two points at which they were

Temperature of Bag.		Temperature of Thermometer.		Temperature of Arm (Calculated)[1]	Δ	
Degrees.	Mean Value.	Degrees.	Mean Value.	Degrees.		Mean Value.
50	**50**	44	**44**	**38**	5.1 cm.	**5.1**
45		41			4.1	
45	**45**	40	**40.5**	**36**	3.6	**3.8**
37		36.2			3.5	
37	**37**	36.2	**36.2**	**35.4**	3.3	**3.4**
33		33			3.9	
32.5		33.5			3.8	
32	**32.5**	33	**33.2**	**33.9**	3.4	**3.7**
21.5		25.5			3.7	
20	**20.7**	23.6	**24.2**	**27.7**	4.1	**3.9**
10	**10**	17	**17**	**24**		**4.6**
5	**5**	13	**13**	**21**		**(6.0)**[2]

[1] Cf., p. 356.

[2] The value given in brackets is not the mean of this series but the maximum. The entire series was as follows:

Down.	Up.
(1) 6.0	(2) 5.2
(3) 5.0	(4) 4.8
(5) 4.4	(6) 4.2

I attribute this constant fall to the effect of warming as I removed the bag to measure the threshold. A control experiment with an ice bag (thermometer between bag and arm 2.1°) gave a threshold beyond 7 cm.

felt as a single point. Each value of Δ is a mean taken from six experiments (occasionally only four). Three of these values were taken by reducing the distance between the points until they were felt as one, and then by increasing them from below the threshold until they were felt as two. On plotting the curve one will find that it apparently has a minimum at 35.4° C. which is produced by water at 37.0° C. Beyond this point it rises much more abruptly than it fell on the other side.

The very steep slope on the side of the curve above the minimum may not be due solely to a decrease in sensibility. The subject found the 50° bag very painful, and each stimulus was like two needles going deep into the skin. Consequently it was harder to attend to the character of the stimulus and more difficult to recognize the points as single or double.

To what, we may ask, is the influence of temperature on the spatial threshold due? The answer to this is that temperature varies the sensibility of the skin. Consequently, it weakens or strengthens the stimulus. Weak stimuli are less, and strong more readily perceived as double. Therefore the variation of the spatial threshold with temperature expresses the variation in the strength of the stimulus due to the temperature of the skin. This conclusion rests upon two statements: (*a*) The spatial threshold is a function of the intensity of the stimulus; (*b*) the touch threshold is a function of temperature.

The following sections will make these points clear and also show that the touch threshold varies in the same way as the spatial threshold with temperature.

IV. Influence of the Ppessure of the Points on the Spatial Threshold.

In order to investigate the influence of the pressure of the points on the spatial threshold I took an æsthesiometer to which I attached much weaker bristles than those above described. The experiments given below were made on the inner side of the first phalanx of the middle finger. Those quoted are for subject H. The values given represent millimeters.

In judging of the value of these results one may use the following method: Let us suppose that the experiments with

NORMAL (*i. e.*, WITHOUT BAG ON THE HAND).

Strong.		Weak.	
Down.	Up.	Down.	Up.
3	4	4.5	5
2	3	3.0	5
5	7	7.5	10
Mean.............3.0 mm.		Mean...............4.4 mm.	

BAG ON HAND CONTAINING WATER AT 30°.

Strong.		Weak.	
Down.	Up.	Down.	Up.
4	3	5	6
3	3	4	6
7	6	9	12
Mean..............3.2 mm.		Mean...............5.2 mm.	

BAG ON HAND CONTAINING WATER AT 50°.

Strong.		Weak.	
Down.	Up.	Down.	Up.
2	5	4	7
3	4	5	6
3	4	5	6
8	13	14	19
Mean...............3.5 mm.		Mean...............5.5 mm.	

weak bristles constitute a kind of normal with which the observations with strong bristles are to be compared. In any set of experiments the individual readings for strong bristles would then by the law of probabilities exceed and fall short of the mean for weak bristles an equal number of times. The proba- of one excess of the weak over the strong would be ½ : for two successive excesses ¼ etc. In the 14 experiments given all of the individual readings for the strong are below the means for the weak. The probability of this happening by chance is $1/2^{14} = 1/16384$.

An apparent difficulty is presented by the fact that the individual readings are of two kinds "up" and "down." But while the "downs" have a tendency to fall short of the mean this is counterbalanced by an opposite tendency of the "ups" to exceed the mean. There is therefore very strong evidence to show that the spatial threshold is a function of the pressure of the stimulus. Just what law it follows I have not investigated.

V. Influence of the Temperature of the Skin on the Touch Threshold.

The instruments used in investigating the touch threshold were a set of von Frey æsthesiometers with sliding tubes. These I carefully graduated on a delicate balance. Practice in graduating the hairs is, I think, an indispensable condition for accurately using them in the actual experiments. The first trials at the balance seem hopeless, and if one touched the skin in the same way as he is likely at first to touch the pan of the balance, no constant results could be obtained. By acquiring the proper delicacy of touch in graduating the hairs it becomes possible to perform accurate experiments.

The diameter of the hairs was measured by means of a microscope. Below are given the tables of standardization for the æsthesiometers most frequently used. The measurement of the bending power is one of great difficulty. Care must be taken not to breathe on the pans of the balance. Some sort of screen must be put between the face and the balance while measuring the bending power. With thin long hairs the minimum bending power is indicated by a very slight movement, which is scarcely perceptible. For stiffer hairs one is aided by a slight sound of the weight pan falling back into place. Only the medium lengths of the hairs give accurate measurements.

Æsthesiometer I.

Diameter = .115 mm. / .090 mm. Radius = .0575 mm. / .0450 mm.

2 | .1025 mm.

Mean radius = .052 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
20	0.006[1]–7	0.115 gm.	32	0.009–*10*	0.192 gm.
25	0.006–*7*	0.135 gm.	33	0.011–*12*	0.231 gm.
26	——	(0.137 gm.)[2]	34	0.018–*19*	0.366 gm.
27	——	(0.140 gm.)	35	0.023–24	0.452 gm.
28	——	(0.142 gm.)	36	0.031–*32*	0.606 gm.
29	——	(0.148 gm.)	37	0.050–*51*	0.980 gm.
30	0.007–*8*	0.154 gm.	38	0.070–*80*	1.539 gm.
31	0.008–9	0.173 gm.			

[1] Numbers in italics indicate the bending power to which the hair more nearly approaches.

[2] Values given in parentheses are obtained by plotting a curve, the scale reading giving the abscissas, and the bending power, or bending power divided by mean radius, the ordinates.

ÆSTHESIOMETER III[a].

Diameter = .150 mm. .110 mm.

Radius = .075 mm. .055 mm.

2 | .130 mm.

Mean Radius = .065 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
0	0.*040*-42	0.601 gm.	25	0.*075*-80	1.154 gm.
5	0.045	0.692 gm.	28	(0.084)	(1.291 gm.)
10	(0.052)	(0.800 gm.)	30	0.090-*95*	1.461 gm.
15	0.059-*60*	0.907 gm.	31	0.110-*115*	1.769 gm.
20	(0.67)	(1.030 gm.)	32	0.135-*145*	2.230 gm.
22	(0.070)	(1.077 gm.)	33	0.175-*180*	2.769 gm.
23	(0.072)	(1.110 gm.)	34[1]	0.230-*235*	3.615 gm.
24	(0.074)	(1.138 gm.)			

[1] Beyond this point the hair could not be accurately callibrated.

ÆSTHESIOMETER III[c].

Diameter = .1805 mm. .1881 mm.

Radius = .09025 mm. .09405 mm.

2 | .18430 mm.

Mean radius = .09215 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
0	0.035-*40*	0.434 gm.	15	0.130-*135*	1.466 gm.
5	0.050-*55*	0.597 gm.	20	0.225-*230*	2.497 gm.
10	0.080-*85*	0.923 gm.	25	0.640-*650*	7.106 gm.

ÆSTHESIOMETER IV.

Diameter = 0.328 mm. 0.200 mm.

Radius = 0.164 mm. 0.100 mm.

2 | 0.264 mm.

Mean radius = 0.132 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
0	0.*090*-95	0.689 gm.	25	(1.55)	(11.7) gm.
5	0.135-*140*	1.061 gm.	27	(3.00)	(22.8) gm.
10	0.275-*280*	2.121 gm.	30	3.250-*3.300*	25.000 gm.
15	0.440-*450*	3.401 gm.	40	Beyond 10.0 gr.	Beyond 75 gm.
20	0.820-*830*	6.280 gm.			

The method of cooling and heating the skin employed in the previous experiments was used in this. The inner fore-arm was the place experimented on. A touch spot was sought in this locality and marked with a capillary glass tube containing a solution of methylene blue.

Below are given the laboratory notes for the experiments with one subject. They are followed by a table of maximum values for the thresholds at the various recorded temperatures. The maximum values are preferable because of the error that is essential to the method I employed. In lifting the bag to take the threshold the skin cools off or warms up a little before the actual measurement can be taken. Consequently the measurements in general are a little below what they should be.

SUBJECT F: 13 Jan., 1910.

Normal threshold æs. I. .35–.36 = 0.606–0.980 gm.

3:47 p. m.	Bag 50° placed on arm.
3:52–3:56	Threshold æs. I. 39–40 = beyond 1.5 gm.
3:59	Bag cooled to 46°. Replaced by another 50°.
4:02–4:05	Threshold æs. IIIª 20–25 = 1.030–1.154 gm.
4:10	Bag replaced by another 50°.
4:12–4:14½	Threshold æs. IIIª 22–24 = 1.077–1.138 gm.
4:15	Bag removed and not replaced.
4:20–4:23	Attempted measurement with æs. IIIª fails. Threshold æs. I. 35–36 = 0.606–0.980.
4:25–427	Threshold æs. I. 35–36 = 0.606–0.980 gm.

SUBJECT F: 14 Jan., 1910.

Normal threshold æs. I. *32*[1]–33 = *0.192*–0.231 gm.

3:25 p. m.	ICE BAG placed on arm.	
3:30–3:32	Attempted measurements with æs. IIIª, doubtful at 40°.	
3:35–3:40	Attempted measurement with æs. IV, subject felt hard pressure at 40 which is beyond 75.0 gm.	
3:41	Ice bag removed and not replaced.	
3:42–3:42½	Æs. IIIª	30–35 = 1.461–beyond 3.6 gm.
3:43–3:43½	Æs. IIIª	5–15 = 0.692–0.907 gm.
3:44–3:45	Æs. I	35–37 = 0.452–0.980 gm.
3:46–3:46½	Æs. I	34–35 = 0.366–0.452 gm.
3:48–3:48½	Æs. I	33–34 = 0.231–0.366 gm.
3:50–3:52	Æs. I	36–37 = 0.606–0.980 gm.
3:55–3:55½	Æs. I	36–37 = 0.606–0.980 gm.
3:56–3:58	Æs. I	36–37 = 0.606–0.980 gm.

SUBJECT F: 14 Feb., 1910.

Bag 10° placed on arm and replaced several times by a fresh bag 10° during 10 minutes. The temperature registered by a thermometer placed between bag and arm was 15.4°. Threshold measured at end of the 10 minutes was æs. IIIª 33–34 = 2.769–3.615 gm.

3:35 p. m. Bag warmed to 11.2°; replaced by another 10°.

[1] Values in italics mean that the threshold is nearer that limit than the other. This can sometimes be judged by the number of correct guesses or by the subject saying that the sensation perceived was very clear or scarcely noticeable.

3:39	Threshold æs. III^a 30–31 = 1.461–1.769 gm.
3:41	Bag warmed to 11.2° ; replaced by another 10°.
3:44	Threshold æs. III^a 32–33 = 2.230–2.769 gm.
3:48	Bag warmed to 11.2° ; replaced by another 10°.
3:53	Threshold æs. III^a 31–32 = 1.769–2.230 gm.
3:53	Bag removed and not replaced.
4:07	Threshold æs. I. 31–32 = 0.173–0.192 gm.

SUBJECT F: 10 Mar., 1910.

Normal threshold æs. I. 33–34 = 0.231–0.366 gm.

3:34 p. m.	Bag **33.5°** placed on arm.
3:50–4:00	Threshold æs. I. 35–*36* = 0.452–0.*606* gm.
	Threshold æs. III^a 0–5 = 0.601–0.692 gm.
	Temperature of bag at end of experiment 31.7°. Temperature between bag and arm at end of experiment 32.6°.
4:20	Bag **50°** placed on arm.
4:38	Threshold æs. III^a 28–30 = 1.291–1.461 gm.
	Temperature of bag at end of experiment 46.8°. Temperature between bag and arm at end of experiment 42.9°.

SUBJECT F: 15 Mar., 1910.

Normal threshold æs. I. *33*–34 = *0.231*–0.366 gm.

2:16 p. m.	Bag **33°** placed on arm.
2:25	Bag cooled to 32.1° ; replaced by another 33°.
2:30	Threshold æs. I. 34–*35* = 0.366–0.452 gm.
2:35	Temperature between bag and arm 33.6°, temperature of bag 32.3°.
2:35½	Bag **25°** placed on arm.
2:42	Bag cooled to 24.8° ; replaced by another 25°.
2:46	Threshold æs. I. 35–36 = 0.452–0.606 gm.
2:53½	Threshold æs. III^a *25*–30 = *1.154*–1.461 gm.
2:57	Temperature between bag and arm 28.4°, temperature of bag 24.6°.
2:58	Bag replaced by another 24.6°.
3:00	Threshold æs. III^a 25–*30* = 1.154–*1.461* gm.
3:04	Temperature between bag and arm 28.2°. temperature of bag 24.6°

SUBJECT F: 22 Mar., 1910.

Normal threshold æs. I. 35–*36* = 0.452–*0.606* gm.

2:16 p. m.	Bag **34°** placed on arm.
2:21½	Bag cooled to 32.2° ; replaced by another 34°.
2:24	Threshold æs. I. 35–*36* = 0.452–*0.606* gm.
2:28	Temperature between bag and arm 34.5°, temperature of bag 33.4°.
2:29	Bag **37°** placed on arm.
2:32	Bag cooled to 36.4° ; replaced by another 37°.
2:34½	Threshold æs. I. 34–*35* = 0.366–*0.452* gm.
2:38	Bag cooled to 36.4° ; replaced by another 37°.
2:42½	Threshold æs. I. 33–*34* = 0.231–*0.366* gm.
2:45	Temperature between bag and arm 36°, temperature of bag 36.5°.
2:48½	Threshold æs. I. *34*–35 = *0.366*–0.452 gm.
2:50	Temperature between bag and arm 36.2°, temperature of bag 36.6°.
2:51½	Bag **40°** placed on arm.

2:57	Bag cooled to 39.3°; replaced by another 40°.
3:00	Threshold æs. I. 33-*34* = 0.231-*0.366* gm.[1]
3:03½	Bag cooled to 39.0°; replaced by another 40°.
3:08½	Threshold æs. I. 34-*35* = 0.366-*0.452* gm.
3:11½	Temperature between bag and arm 37.8°, temperature of bag 39.1°.
3:12	Bag **45°** placed on arm.
3:16	Bag replaced by another 45°.
3:20	Threshold æs. I. 35-*36* = 0.452-*0.606* gm.
3:25	Bag cooled to 43.1°; replaced by another 45°.
3:30	Threshold æs. I. 34-35 = 0.366-0.452 gm.
3:33½	Temperature between bag and arm 40.2°, temperature of bag 43.2°.
3:34	Bag **50°** placed on arm.
3:40	Bag replaced by another 50°.
3:42½	Did not feel æs. I. 36 = 0.606 gm.
3:49	Bag cooled to 48°; replaced by another 50°.
3:54	Threshold æs. III^c 20-25 = 2.497-7.106 gm.
3:58	Temperature between bag and arm 42.5°, temperature of bag 48.2°.
4:01	Threshold æs. III^c 20-25 = 2.497-7.106 gm.
4:04	Bag **34°** placed on arm.
4:10	Threshold æs. III^c 0-5 = 0.434-0.597 gm.
4:13	Bag cooled to 33.4°; replaced by another 34°.
	Threshold æs. I. does not feel 37 = 0.980 gm.
	Threshold æs. III^c 15-20 = 1.466-2.497 gm.

TABLE OF MAXIMUM VALUES.

Temperature of Bag. Degrees.	Temperature Between Bag and Arm. Degrees.	Calculated Temperature of Arm. Degrees.	Maximum Threshold.
0	—	—	Beyond 75 gm.
10	15.4	20.8	3.615 gm.
24.6	28.4	32.2	1.461 gm.
33.4	34.5	35.6	0.606 gm.
36.6	36.2	35.8	0.452 gm.
39.1	37.8	36.5	0.452 gm.
43.2	40.2	37.2	0.606 gm.
46.8	42.9	39.0	1.461 gm.

The assumption I have made in calculating the temperature of the arm is that the temperature of a thermometer placed between the skin and a hot water bag approximates the arithmetical mean between skin temperature and that of the hot water bag. In the region of the minimum threshold the error of this assumption can scarcely exceed 1° centigrade. There are not wanting experiments to test the accuracy of this assumption M. J. Lefévre by means of a specially constructed thermopile measured the temperature of the skin two millimeters below

[1] Felt a touch once only at 33, always at 34.

the surface, and also the surface temperature of the skin in baths of 5°, 12°, 18°, 24° and 30° C. If his results are correct, in calculating his observed temperature of the skin below the surface from the observed surface temperature and that of the bath, my assumption would lead to the following errors:

5° C. Minus.	12° C. Minus.	18° C. Minus.	24° C. Minus.	30° C. Minus.
−3.6° C.	−3.1° C.	−2.1° C.	−1.1° C.	−0.5° C.

Since, however, it appears that Lefévre measured his temperatures in all cases after a constant interval of time, and since the longer the arm stays in the cold bath the lower will be its temperature until an equilibrium is established, there is an increasing error in his results as he goes away from physiological zero. This being taken into consideration, the assumption I have made cannot be far away from the reality.[1]

In spite of the rough character of these experiments the numerical data are not without value. They afford us some very definite information about the influence of temperature on sensibility. From zero on to a certain point, which I have not exactly determined, sensibility increases with the rise of temperature. Beyond this point it decreases. The temperatures of the bag on either side of this maximum were 33.4° and 43.2°. The readings of a thermometer placed between the bag and the arm on either side of this maximum were 34.5° and 40.2°. It is clear therefore that the maximum lies above 34.5° and below 40.2°. If we assume that the temperature of the thermometer between the bag and the arm is the arithmetical mean between the temperatures of the bag and the arm, then the point of maximum sensibility lies between 35.8° and 37.2°. Allowing for an error of about 8° centigrade on either side we may say with fair probability that the temperature of maximum sensibility lies between 35° and 38° C. This result that the sensibility of the skin increases with rising temperature to a maximum and then decreases, has been confirmed with other

[1] Cf. M. J. Lefévre, 'Étude expérimentale du pouvoir protecteur de la peau et des ses coefficients de la conductibilite,' *Journal de physiologie et de pathologie générale*, 1901, III., pp. 1–14.

subjects. Von Frey[1] failed to find that cooling had any influence on the sensibility of the skin. His method of cooling however was the use of a water bath at 8° C. It is rather remarkable that he did not and his failure must have been due to some oversight. The water bath however is a bad method of cooling because it seriously affects the condition of the skin.

VI. Influence of the Direct Current on the Sensibility of the Skin.

As early as 1863 Fräulein Nadjeschda Suslowa found that the sensibility of the skin was increased in the neighborhood of the kathode and decreased around the anode. Professor Setchenow, however, in communicating her results to the *Zeitschrift für rationelle Medicin*, added a note in which he said that he had repeated his student's work but could obtain no definite results. He thought, however, that this was due to his lack of experience in this line of work, and left the matter entirely to the trustworthiness of Fräulein Suslowa's work. A complete analysis of Fräulein Suslowa's results is given below (pp. 365 ff.). I have repeated her experiments with some modifications.

Robert Graeber[2] confirmed the results of Fräulein Suslowa for the spatial threshold. He refers to the writings of other authors on this problem, but abstains from giving any references beyond their names. Graeber rejected Nadjeschda Suslowa's method of experimenting on the basis of variable results obtained by Carl Spanke. Spanke found that on applying two electrodes to the skin he would at times get the results found by Fräulein Suslowa, but then again he would get a decrease of sensibility at both electrodes, and again an increase of sensibility at both electrodes. In the beginning I was annoyed by the same fluctuations, but I believe now that I can obtain Fräulein Suslowa's results at any time by using the proper strength of current. Just as in the motor nerves so also in the skin with weak currents the cathodic effect extends over to the anode, with strong currents the anodic effect extends over to the cath-

[1] 'Untersuchungen über die Sinnesfunctionen der mensch. Haut,' *Abh. der M. phys. Classe der K. S. Gesellschaft der Wiss.*, XXIII., p. 221.

[2] 'Untersuchungen über den Einfluss galvanischer Ströme auf den Tastsinn der Haut' (Diss., Bonn, 1884, pp. 21).

ode. If one bears that law in mind and also remembers that the electrolysis of skin produces at the anode a substance that is not readily absorbed, he will have no difficulty in verifying Fräulein Suslowa's law of the electrotonus of the skin.

Graeber obtained constant results by placing the arm in a water bath of 27° R. One electrode was placed on the chest, the other in the water bath. The essential feature of this procedure really consisted in nothing more than a weakening of the current by the introduction of considerable resistance. Graeber also found as a regular occurrence the phenomenon of 'transfer.' If the sensibility on one arm is increased by the electric current it is simultaneously decreased on the other arm to which no current has been applied, and *vice versa*.

In my experiments I used for zinc bracelets flexible sheet zinc about 1 cm. in width, hinged in the middle and capable of being adjusted to the size of the arm by a set screw. Between this and the arm I wrapped a strip of cotton felt soaked in ordinary tap water or zinc sulphate solution. After placing the bracelets on the arm I found inside of them (*i. e.*, in the path of the current) two touch spots as close as possible to the bracelets. I then measured the thresholds of these spots, after which I turned on the current. The following results were obtained:

SUBJECT B, 31 March, 1910.

Two Edison-Laclede cells.	About 5 cm. between electrodes.
Upper Electrode.	*Lower Electrode.*
Threshold before current was turned on:	
Æs. I. 32-33 = 0.191-0.231	Æs. I. 33-34 = 0.231-0.366
Threshold after current was turned on:	
+	—
Æs. I. 33-34 = 0.231-0.366	Æs. I. 32-33 = 0.191-0.231
Current reversed:	
—	+
Æs. I. 32-33 = 0.192-0.231	Æs. I. 32-33 = 0.191-0.231
Æs. I. 32-33 = 0.192-0.231	Æs. I. 32-33 = 0.191-0.231
Current reversed:	
+	—
Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192

SUBJECT B, 13 May, 1910.

Threshold before current was turned on:	
Æs. I. 34-35 = 0.366-0.452	Æs. I. 33-34 = 0.231-0.366

Threshold after current was turned on :

+	−
Æs. I. 32-33 = 0.366-0.452	Æs. I. 32-33 = 0.192-0.231
32-33 = 0.366-0.452	30-31 = 0.154-0.173
32-33 = 0.366-0.452	29-30 = 0.148-0.154
32-33 = 0.366-0.452[1]	

Current reversed :

−	+
Æs. I. 30-31 = 0.154-0.173	Æs. I. 29-30 = 0.148-0.154
Æs. I. 31-32 = 0.173-0.192	Æs. I. 31-32 = 0.173-0.192
	Æs. I. 32-33 = 0.192-0.231

Current broken for five minutes :
Threshold at end of five minutes :

Æs. I. 34-35 = 0.366-0.452	Æs. I. 33-34 = 0.231-0.366

Current turned on :

−	+
Æs. I. 33-34 = 0.231-0.366	Æs. I. 32-33 = 0.192-0.231
Æs. I. 33-34 = 0.231-0.366	Æs. I. 34-35 = 0.366-0.452

Subject B, 23 May, 1910.

Threshold before current was turned on :

Æs. I. 32-33 = 0.192-0.231	Æs. I. 29-30 = 0.148-0.154
Æs. I. 33-34 = 0.231-0.366	Æs. I. 29-30 = 0.148-0.154
Æs. I. 33-34 = 0.231-0.366	

One cell turned on :

−	+
Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192

Two cells turned on :

−	+
Æs. I. 32-33 = 0.192-0.231	Æs. I. 31-32 = 0.173-0.192

Current reversed :

+	−
Æs. I. 34-35 = 0.366-0.452	Æs. I. 30-31 = 0.154-0.173
Æs. I. 33-34 = 0.231-0.366	Æs. I. 30-31 = 0.154-0.173
Æs. I. 34-35 = 0.366-0.452	Æs. I. 29-31 = 0.148-0.154

Current broken :

Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192

Two cells turned on :

−	+
Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192[2]

Subject F, 1 April, 1910.

Threshold before the current was turned on :

Æs. I. 36-37 = 0.606-0.980	Æs. I. 36-37 = 0.606-0.980
Æs. I. 37-38 = 0.980-1.539	Æs. I. 36-37 = 0.980-1.539

[1] Note the cathodic effect traveling to the anode. The next reversal, however, shows that a true anodic and cathodic effect is present.

[2] Here the experiment had to be interrupted. Had it been continued, anodic and cathodic effects probably would have appeared.

Threshold after current was turned on :

+	—
Beyond æs. I. 38 = 1.539	Æs. I. 35-36 = 0.452-0.606
Beyond æs. I. 38 = 1.539	Æs. I. 35-36 = 0.452-0.606

Current reversed :

—	+
Æs. I. 35-36 = 0.452-0.606	Beyond æs. I. 38 = 1.539
Æs. I. 35-36 = 0.452-0.606	Beyond æs. I. 38 = 1.539

Current reversed :

+	—
Æs. I. 37-38 = 0.980-1.539	Æs. I. 35-36 = 0.452-0.606

Current broken :

Æs. I. 37-38 = 0.980-1.539	Æs. I. 36-37 = 0.606-0.980

SUBJECT F, 24 May, 1910.

Threshold before the current was turned on :

Æs. I. 35-36 = 0.452-0.606	Æs. I. 36-37 = 0.606-0.980

Threshold after current was turned on :

—	+
Æs. I. 34-35 = 0.366-0.452	Æs. I. 32-33 = 0.192-0.231

Current reversed :

+	—
Æs. I. 37-38 = 0.980-1.539	Æs. I. 32-33 = 0.192-0.231
Æs. I. 37-38 = 0.980-1.539	Æs. I. 33-34 = 0.231-0.366

Current reversed :

—	+
Æs. I. 37-38 = 0.980-1.539	Æs. I. 34-35 = 0.366-0.452
Æs. I. 35-36 = 0.452-0.606	Æs. I. 34-35 = 0.366-0.452

Current reversed :

+	—
Æs. I. 37-38 = 0.980-1.539	Æs. I. 33-34 = 0.231-0.366
Æs. I. 37-38 = 0.980-1.539	Æs. I. 35-36 = 0.452-0.606
	Æs. I. 33-*34* = 0.231-*0.366*

Current broken 3:17 p. m.

3:24 p.m. does not feel æs. I. 38 = 1.539	3:22 p.m. æs. I. 35-36 = 0.452-0.606
3:26 p.m. does not feel æs. I. 38 = 1.539	3:25 p.m. æs. I. 36-37 = 0.606-0.980
3:30 p.m. does not feel æs. I. 38 = 1.539	

In the following experiment I investigated with subject 'F.' (1) a spot in the neighborhood of the upper electrode which was the anode throughout, (2) a spot about 1 cm. below the upper electrode, and (3) a spot in the middle between the two electrodes. My object was to study the effect of a weak and strong current.

	1 Spot at Anode.	2 Spot 1 cm. Away from Anode.	3 Spot Half-way Between Anode and Cathode.
Threshold before current was turned on.	Æs. I. 35–36 = 0.452–0.606	Æs. I. 33–34 = 0.231–0.366	Æs. I. 35–36 = 0.452–0.606
Current on (two cells).	Æs. I. 34–35 = 0.366–0.452	Æs. I. 33–34 = 0.231–0.366 (3.00 P.M.) Æs. I. 31–32 = 0.173–0.192 (3.07½ P.M.)	Æs. I. 32–33 = 0.192–0.231

These results show that the cathode effect has traveled far over into the region of the anode.

	1	2	3
Current off 2 minutes 4 cells then turned on.	Æs. I. 35–36 = 0.452 – 0.606	Æs. I. 32–33 = 0.192–0.231 (3.14 P.M.) Æs. I. 33–34 = 0.231–0.366 (3.18 P.M)	Æs. I, 35–36 = 0.452–0.606

From this table it appears that with double the strength of current spot (3) no longer suffers a reduction of its sensibility. Spot (2) with increase of time decreases in sensibility rather than increases as it did with the weak current. Spot (1), however, has returned to its normal threshold without being reduced in sensibility while the current was on.

	1	2	3
Current off at 3.24½ P.M.	3.38 P.M. æs. I. 35–36 = 0.452–0.606	3.36½ P.M. æs. I. 31–32 = 0.173–0.192 3.41 P.M. æs. I. 30–31 = 0.154–0.173	3.34 P.M. æs. I. 34–35 = 0.366–0.452 3.42 P.M. æs. I. 33–34 = 0.325–0.366

Here we see that on breaking the current there was an increased sensibility at the anode judging by spot (2) and (3) which by analogy with the motor nerve was something to be expected. Spot (1) seems to have been not so amenable to the influence of the current. I have several times found similar cases and have explained them on the supposition that the direction of the current varies owing to irregularities of pressure or

moisture at various points of the bracelets. But if one will examine Pflüger's curve for the irritability of muscle one will see that a minimum effect is often to be found in the immediate region of the electrode.

A careful study of all these results will, I think, lead one to the conclusion that Pflüger's law for the irritability of motor nerves and muscles holds also for the skin. Sensibility as well as irritability is increased at the cathode and diminished at the anode.

VII. Influence of the Induced Current on the Sensibility of the Skin.

When we treat an area of the skin with an alternating current applied by means of the electrodes (metallic) of an induction coil, two phenomena are to be observed immediately after removing the electrodes. (*a*) The sensibility of the skin is considerably lowered, (*b*) a spot which was before a touch spot has been transformed apparently into a pain spot.

I will give the laboratory notes for one experiment on this point, which can be verified repeatedly without any trouble. The induced current I used was always somewhat painful. Fräulein Suslowa found a reduction of sensibility even when the current was so weak that it could not be felt.

Subject F, February 10, 1910.

Touch spot feels æs. III^a at 5 = 0.692 gm. clearly.
Induction coil electrode applied.
Æs. III^a 40 with hard pressure feels sharp.
Æs. IV. 27 = 22.8 gm. felt.
Electrode applied again:
Æs. IV. 27 = 22.8 gm. not felt.
When I had finished making the above entry
Æs. IV. 27 was felt.
Electrode applied again:
Æs. IV. 30 = 25.0 gm. felt like the prick of a needle.
When I had finished making entry
Æs. IV. 30 felt like a touch.

I then sought a pain spot. I found one with a threshold, Æs. IV. 15–20 = 3.401–6.280 gm. Touch was felt below 20 but at 20 it felt a little sharp.

Electrodes applied.	Nothing felt at	20	Æs. IV. =	6.280	gm.
" "	Touch felt at	25	"	11.7	"
" "	Nothing felt at	25	"	11.7	"

Electrodes applied.	Sharp pain at	30	"	25.0	"
" "	Nothing felt at	25	"	11.7	"
" "	Sharp pain at	30	"	25.0	"

These results show that the threshold for pain is raised by the induced current and that, too, in proportion to the time in which it acted.

I then sought a touch spot in the neighborhood of the above which I marked as accurately as possible without the use of a lens.

Before the electrodes were applied—

	Touch felt but no pain at Æs. IV. 30	= 25.0	gm.
Electrodes applied.	Pain at Æs. IV. 30	= 25.0	gm.
" "	Nothing felt at Æs. IV. 25	= 11.7	gm.
" "	Pain felt at Æs. IV. 30	= 25.	gm.
" "	Nothing felt at Æs. IV. 25		

From this statement it would seem that the touch spot is capable of transformation into a pain spot by means of the induced current. I thought at first that the reason for this might be that the pain spots in the neighborhood of the touch spot were rendered hypersensitive by the induced current. The result would be that a stimulation of a touch spot by a relatively strong pressure would involve the stimulation of the hypersensitive pain spots in the neighborhood. I therefore made the above test experiment and found that the threshold of the pain spots was also raised by the induced current. Consequently, if after treatment with an induced current a touch spot becomes sensitive to pain it must be because chemical changes have taken place in the touch spot that alter the character of the sensation to which it gives rise. If this is the case, one would expect that the character of a sensitive spot in the skin is determined not merely by anatomical features, but also by chemical compounds or enzymes of some kind that are present in or near the sensitive spots.

In confirmation of the results that the threshold of the pain spot is raised by the induced current, the following experiment may be quoted:

Subject Fe., July 27, 1910.

Threshold of pain spot on inner side, middle, of forearm.

20–25 Æs. IV. = 6.2–11.7 gm.

At 20 Æs. IV. = 6.2 gm. Subject only once felt a sticking sensation.

At 25 Æs. IV. = 11.7 gm. Subject always felt a sticking sensation.
At 30 Æs. IV. = 25.0 gm. Subject felt a sharp sticking sensation.

Electrodes of induction coil applied:

At 25 Æs. IV. = 11.7 gm. Subject feels nothing. After making entry in note-book subject felt a slight stick at æs. IV., 25 = 11.7 gm.

Electrodes applied again:

At 27 Æs. IV. = 25 gm. Subject when touched the first time felt nothing. The second time he felt a touch which perhaps had a faint coloring of pain.

Electrodes applied again:

At 30 Æs. IV. = 25 gm. No pain felt.
35 Æs. IV. = beyond 25 gm. Sharp pain. About one minute later at 25 æs. IV. = 11.7 gm. Subject felt a sharp sticking sensation.

Electrodes applied again:

At 25 Æs. IV. Subject was not certain whether he had been touched or not.
30 Æs. IV. Subject felt a slight touch as if it were something blowing.

This experiment indicates very clearly that immediately after the application of an induced current the threshold of a pain spot is considerably raised.

It will be interesting to append to this account an analysis of Fräulein Suslowa's[1] experiments in this field.

In 1863 Fräulein Nadjeschda Suslowa undertook to see if the then recently discovered laws of electrophysiology applied also to the sensory nerves of the skin.

She applied the electrodes of an induction coil to the back of the hand. The primary current was constantly interrupted. The strength of the current from the secondary coil was not, however, sufficient to be felt. She then used a human hair head to draw across the skin between the electrodes. With the current on the hair could not be felt. When it was turned off it could be felt. It could also be felt when drawn across the skin outside the area between the electrodes whether the current was on or off. The results were the same even when the current was increased to the point where it could be felt. In explanation of this phenomenon Fräulein Suslowa writes, "The results obtained can perhaps be thus explained: The stroking of the hand with the hair could be felt before the electrical stimulation because the touch stimulus affected only

[1] 'Veränderungen der Hautgefühle unter dem Einflusse eletrischer Reizung.' *Zeitschrift für rationelle Medicin*, III., Reihe, XVII., 1863, pp. 155–160.

individual points of the sensitive skin. But where a relatively large sensitive area is stimulated electrically (the stimulation, indeed, exists but not consciously) the touch stimulus is distributed over this entire area and consequently is too weak to be felt at any individual point" (p. 156). Fräulein Suslowa also investigated the effect of the interrupted current on the spatial threshold. To do this she made use of a compass whose upper portion was of ivory and whose tips were copper, and could therefore be used as electrodes. She then approximated the points of the compass till they were just above the spatial threshold. This being done, she turned on the current. The two points no longer appeared double but single. This result was especially clear on the tip of the tongue. Further experiments showed that the wider apart the points of the compass are placed the stronger must be the current in order that they may appear single. In explanation the author writes, "these phenomena are easily explained from the standpoint of Weber's theory. The electrical current stimulates all the circles of sensation lying between the points of the compass. But according to Weber's theory, in order that a double sensation may take place some of these circles must remain unstimulated" (p. 157). In order to test her theory on this point, she stimulated the area between the points with a brush and found that then too the double sensation disappears. In less sensitive portions of the skin in order to make the double sensation disappear one need not stimulate with the brush the area just between the points but may also stimulate an area pretty far (in ziemlich grosser Entfernung) away—but in a direction perpendicular to a straight line between the points of the compasses. In order to exclude the objection that such stimulation acted as a mere distraction of the attention, she stimulated areas that were on another part of the body from that of the double sensation and found that in such cases the double sensation rarely disappeared but simply became less clear.

Fräulein Suslowa also investigated the effect of the constant current. The current was supplied by two Bunsen cells. As electrodes she used zinc plates of various shapes which were laid on the skin by means of two linen bands saturated with

sulphuric acid. In circuit were a rheostat and a commutator. She first investigated the effect of the current in the region of the positive and negative poles. She placed two zinc bracelets around the forearm and with the brush stimulated the skin now in the region of the positive electrode now at the cathode. "The results with every strength of current remained the same: lowering of sensibility at the anode and heightening of it at the cathode. In order to do away with the objection that possibly this result is due to stimulating with the brush areas which, as a matter of fact, have a different sensibility, one has only to reverse the current" (p. 159). Similar results were obtained with thermal sensations. The author was able with the direct current to duplicate the results obtained for double sensations with the induced current—but only on the tongue. She also found that the power of discriminating between two points was heightened at the negative pole and lowered at the positive. Here Professor Setchenow, under whose directions the experiments were made, entered the following note: "I must acknowledge that on repeating these experiments on my own arm I could obtain no definite results. But since I have in the matter no such rich experience as Miss Suslowa, the fact must be left to her responsibility" (pp. 159–160). In conclusion the author mentions a final fact: "I have discovered that when one dips the arm into any indifferent liquid (*e. g.*, water or oil) which is of the same temperature as the arm the fineness of discrimination between two points is considerably increased" (p. 160).

Summary of Results.

A. *Influence of the Temperature of the Skin on the Spatial Threshold.*

1. The spatial threshold varies with the temperature of the skin, decreasing from the lower temperatures on up to a point not far from 36° C. Beyond this point it increases with the rising temperature of the skin.

2. The reason for this is to be sought for in the fact that the change in the spatial threshold simply expresses the variation in the intensity of the stimulus due to the temperature of the skin.

B. *Influence of the Pressure of the Points on the Spatial Threshold.*

3. It is in general true that at various temperatures of the skin a weak pressure of the points gives a larger spatial threshold than a strong pressure.

C. *Influence of the Temperature of the Skin on the Touch Threshold.*

4. The minimum threshold of touch is found when the temperature of the skin is between 35° C. and 38° C. Above and below these points the sensibility of the skin decreases.

D. *Influence of the Direct Current on the Sensibility of the Skin.*

5. Pflüger's law for the irritability of motor nerves and muscles holds also for the skin, so that the sensibility of the skin is decreased at the anode and increased at the cathode.

6. When the current is too strong this result may be obscured by the creeping of the anodic effect over into the region of the cathode.

7. When it is too weak it may be obscured by the reverse process — the creeping of the cathodic effect over into the region of the anode.

E. *Influence of the Induced Current on the Sensibility of the Skin.*

8. *Immediately* after the treatment of an area of the skin with the induced current the sensibility is considerably lowered. This is true, not only for touch spots, but also for pain spots.

9. A touch spot is under these conditions transformed into a pain spot.

Theoretical Interpretations.

1. *Sensibility as a Function of Dissociation.*

As stated in the introduction, I was led to investigate the relation between the temperature of the skin and the threshold of touch by the conjecture that it might bear some relation to the law that governs the velocity of chemical reactions at different temperatures. The threshold might depend upon the velo-

city of a chemical reaction in this way: Suppose that a change in pressure disturbs the conditions of equilibrium in the chemical reactions that are constantly taking place in the tissues. When this equilibrium is disturbed some reactions will go on faster or slower than before. The result will be the production of some definite compound or compounds concerned in the stimulation. There must be a minimum amount of this compound that corresponds to the threshold of sensation. If this is the case, then the amount produced will vary with the temperature and time of stimulation. Since the time of a momentary stimulus will average up as a constant, then for the lower degrees of temperature a greater pressure would be needed and therefore a greater disturbance of chemical equilibrium in order to produce a threshold stimulation. The investigation of this problem would be of great theoretical interest, but I do not as yet see the way to solve it on the basis of my present crude data. Furthermore, the curve showing the relation between the sensibility of the skin and its temperature has a maximum between 35 and 38° C. It is not likely that the velocity of reaction would show a maximum at this point. This difficulty might be explained by supposing that the stimulation is due to the formation of some compound which commences to break up or coagulate or precipitate at temperatures above that of the maximum of the curve obtained.

But there are other factors besides the velocity of reaction to be taken into consideration, and some one of them perhaps may be the dominant one in the variation due to temperature. Among these factors that of dissociation is of prime importance. The steps by which I have been led to this conclusion are as follows:

(*a*) The concentration of certain ions — especially Na and Ca, are known to be of great importance in the phenomenon of muscular irritability.[1] It might also be of importance in the sensibility of the skin. One way of varying the concentration of the ions in the tissues is by influencing, through changes in temperature, the degree of dissociation.

(*b*) As a general rule dissociation increases with rising temperature, but it has been shown by Arrhenius[1] that under certain

[1] Cf. Loeb, *The Dynamics of Living Matter*, Lecture V.

conditions there must be a maximum temperature beyond which the degree of dissociation decreases. This he showed to be actually the case, and consequently the sensibility of the skin might show a maximum at the point of maximum dissociation — if it were dependent on the concentration of some ion or ions in the tissues.

(*c*) The next step would be to actually measure the degree of dissociation in the blood and tissues at various temperatures. Since dissociation varies directly as the conductivity, this measurement is not difficult in animals like the frog and turtle, whose organs and muscles can function for some time after excision. This work has been done by G. Galeotti[2] for the frog and turtle. In living tissues he found an increase of conductivity from 12° on to a point at which he said the tissue commenced to die. The conductivity then decreased until coagulation, after which it rapidly increased with rising temperatures. The point of the first maximum varied with the tissue from 29°–35°. Dead tissues do not show this maximum and minimum, but only a constant increase along a parabolic curve.

The actual maxima for the different organs were:

The kidneys, about	32°.
Liver, between	32° and 34°.
Spleen, "	30° " 32°.
Muscles, "	28° " 30°.

Other interesting results were the following: After an organ has been removed from the body its conductivity decreases until a minimum is reached, after which it commences to increase. When decay sets in the conductivity reaches very high values. After the tissue has been killed by heat or cooling the conductivity decreases. The contractility of a muscle seems to be a function of its conductivity. The conductivity of the blood continues to increase with rising temperature even after coagulation.

The maximum and minimum values in the curve of conductivity for living tissues under the influence of heat, may be due, says Galeotti, to one of two causes —

[1] 'Ueber die Dissociationswarme und den Einfluss der Temperatur auf den Dissociationsgrad der Elektrolyte,' *Zeitschrift für physikalische Chemie*, 1889, IV., pp. 96–116.

[2] 'Ueber die elektrische Leitfähigkeit der tierischen Gewebe,' *Zeitschrift für Biologie*, 1902 (XLIII.), XXV., pp. 289–340.

The data obtained for the muscles were as follows:

Temperature. Degrees.	Cross Cut Frog Muscle. Living Tissue.	Longitudinally Cut Muscle of the Turtle. Living Tissue.	Gastrocnemius of the Frog. Living Tissue.	Muscles of the Turtle. Dead Tissue.
14	102.9	——	——	——
16	111.3	——	——	——
18	112.3	——	——	——
20	114.4	——	——	——
22	116.6	66.4	32.7	——
24	124.2	69.9	34.2	(72.2)
26	126.3	74.7	35.2	80.4
28	128.2	78.3	36.1	84.8
30	130.6	71.1	39.0	89.1
32	130.4	75.3	38.1	96.1
34	130.4	75.6	37.8	102.3
36	130.3	74.7	37.4	110.7
38	124.4	72.0	36.8	(123.0)
40	158.9	74.1	35.7	(134.8)
42	176.1	77.4	38.0	145.4
44	194.2	88.2	43.1	153.2
46	206.8	93.0	44.8	178.3
48	216.0	106.5	47.4	(193.0)
50	226.6	129.6	52.1	205.8
52	236.0	——	——	(219.0)
54	256.0	——	——	242.1
56	282.1	——	——	(269.0)
58	378.4	——	——	(306.2)
60	708.0	——	——	(355.0)
62	1010.0	——	——	409.5

(*a*) The internal friction of the protoplasm is increased after death by the coagulation of colloidal bodies. The conductivity of the colloids would thus be decreased and therefore the entire conductivity lowered.

Against this view he mentions the fact that experiments by Arrhenius,[1] Tietzen-Hennig,[2] Lüdeking[3] and Levi[4] show that coagulation does not introduce any marked changes in conductivity. The author himself, and also Sabbatini,[5] found that the conductivity of the serum is not changed after coagulation.

[1] 'Contributions to our Knowledge of the Action of Fluidity on the Conductivity of Electrolytes' (translation of Arrhenius' article by Professor Ramsey). Report of the British Association for the Advancement of Science (1886), 1887, pp. 344–348.

[2] 'Ueber scheinbar feste Electrolyte,' *Annalen der Physik. und Chemie*, 1888, XXXV., pp. 467–475.

[3] Lüdeking, Ch., 'Leitungsfähigkeit gelatinshaltiger Zinkvitriollösungen,' *Annalen der Physik und Chemie*, 1889, XXXVII., pp. 172–176.

[4] Levi (I have not seen this article), *Gazz. Chim. ital.*, 1900, XXV., ii., p. 64.

[5] Louis Sabbatini, 'Détermination du point de congélation des organes animaux,' *Journal de physiologie et de pathologie générale*, 1901, III., pp. 939–950.

(*b*) The second possibility is that the number of free ions in soluton is decreased. As to the way in which this is done he suggests that as the protoplasm dries the ions are combined with the protein molecule and can therefore no longer assist in the conduction of the current. The life of the protoplasm would therefore be characterized by its higher ionization.

On this theory could be explained:

(*a*) The initial rise in the curve as due to the increased mobility of the ions brought about by the rise in temperature—their number remaining constant.

(*b*) The maximum at which the protoplasm commences to die and enter into combination with the ions. That this point is not sharp in the kidney and liver is due to the fact that these organs possess in their intercellular space very rich stores of ions.

(*c*) The fall in the curve is due to the continuation of the process of combination between the protoplasm and the ions.

(*d*) The subsequent rise of the conductivity curve which is close to the point of coagulation, might be due either to the fact that (*a*) the compound between the protoplasm and the ions is suddenly broken up, or that (*b*) the coagulation of the protoplasm breaks the cell walls and membranes of the tissues, thus allowing the ions to move with greater freedom.

. . . .

If such salts as Na and Ca exist to any great extent as free compounds in the tissues of the skin and are not combined with complex proteid molecules in the living tissues, it is scarcely likely that the maxima found by Galeotti are due to conditions that determine the maximum in the curve of Arrhenius. Such salts increase in conductivity on up to very high temperatures. If the conductivity of the tissues has a maximum between 35 and 38° C. the main carrier of the current—to be subject to the law of Arrhenius—must have its maximum somewhere between those limits. This could only be the case if the salts of Na and Ca did not dissociate as independent compounds but because they were in some way taken up as constituent parts of a complex molecule in the living tissues.

Whatever may be the explanation of the phenomena the

fact remains that the degree of dissociation in various organs and tissues of the frog and turtle manifests a maximum in the neighborhood of 30° C. We have also found in the human skin a maximum sensibility between 35 and 38° C. Considering the fact that Galeotti was dealing with cold-blooded animals it is very likely that the maximum conductivity in the human tissues will fall within the limits of the minimum threshold. It is not, of course, logically demonstrated that because two things rise and fall together that therefore they are interdependent. But it constitutes very strong evidence that they are. Within the limits of the temperatures investigated by us one has but to apply a correction of temperature for the difference between warm and cold blooded tissues to see that sensibility follows the general course of the changes in the degree of dissociation. We may therefore lay down the law that *the tactual sensibility of the skin is a function of the degree of dissociation in the tissues.*

The Relation between Sensibility and Irritability.

Not only is the sensibility of the skin at a maximum at the temperature of greatest dissociation, but also the irritability of a muscle. The experiments of Marey placed the maximum irritability of a muscle between 30° and 35° C. Gad and Heymans placed the maximum at 30° C. which is the maximum of conductivity in the above quoted tables of Galeotti's for the gastrocnemius of a frog.

Charles L. Edwards,[1] experimenting on the irritability of frogs' muscle and nerve, found a maximum for muscle between 32.75° and 39.25°. For curarized excised muscle he found maximum contractions at temperatures varying from 33.25° to 38.25°. For non-curarized excised muscle, at 36.25°, 29.25°, 37.75°, and 38.25°. The value 29.25° he looked upon as abnormal. He referred to Marey's [2] results, who obtained maximal contractions between 30° and 35°. Both investigators used

[1] 'The Influence of Warmth upon the Irritability of Frogs' Muscle and Nerve,' *Studies from the Biological Laboratory, Johns Hopkins University,* Baltimore, 1887–1890, IV., pp. 20, 35.

[2] E. J. Marey, *Du Mouvement dans les fonctions de la vie,* Paris, 1868, p. 353.

the same method — enclosure of the muscle in a double-walled cylinder through which water flowed. It is barely possible that Edwards did not allow time for the muscle to assume the temperature of the air in his cylinder, and thus thought it was at a higher temperature than it actually was. Marey's results are close to the maximum conductivity obtained by Galeotti.

Gad and Heymans[1] found that the frog's muscle gave its maximum at 30° both for isotonic and isometric contractions. For tetanic contractions they found the absolute maximum also at 30°, and a second relative maximum at 19°. The latent time they found to decrease constantly with rising temperature. The duration of the contraction also decreased with rising temperature and in accordance to a regular law, the curve being asymptotic to the axis of abscissas and perpendicular to that axis at 5°.

They were of the opinion that the two maxima they found pointed to the existence of two chemical processes involved in muscular contraction: *e. g.*, the formation of lactic acid and the breaking down of lactic acid into CO_2 and H_2O.

One might suggest in passing that there are two phenomena in contraction, (*a*) that of irritability and (*b*) that of contractility. The irritability is a function of conductivity, having its maximum at 30°, as was found by Galeotti. The contractility is a function of the combustion of materials in the tissues dependent upon the amount present and the laws to which the chemical reactions involved in their combustion are subject.

Looking upon the results of Marey, Gad and Heymans as representing the true maximum of irritability (the temperatures given by Edwards being a little too high), we may lay down the law that *the maximum irritability of a muscle is at its temperature of highest dissociation*. This law is exactly analogous to the law of sensation. The two may be expressed as one by simply saying that *maximum sensibility for touch and the maximum of muscular irritability are both found at the temperature of greatest dissociation*.

[1] 'Ueber den Einfluss der Temperatur auf die Leistungsfähigkeit der Muskelsubstanz,' *Archiv für Anatomie und Physiol.* (Physiol. Abtheilung), Suppl. Band, 1890, pp. 59–115.

We have furthermore demonstrated that Pflüger's law for the effect of the constant current on muscles and nerves holds also — as has been hitherto suspected — for the sensibility of the skin; so that sensibility and irritability are both increased at the cathode and decreased at the anode. The decrease of sensibility at the anode might be due to an increase in the concentration of complex ions or, and this is more likely, because of a precipitate [1] found at the anode which is not easily disposed of by the circulation of the blood. At the cathode the increased sensibility is due to ions which migrate to the cathode and are of themselves sufficient to cause by their greater concentration a heightened sensibility, or can do so by entering into more complex, perhaps, proteid compounds.[2] The identity of the phenomenon in muscle, nerve, and sensory end organs teaches the important lesson that between these three tissues there is a fundamental relationship. This relationship consists in the fact that their diverse functions are dependent upon one and the same chemical phenomenon — the concentration of the ions in muscle, nerve and skin.

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BLIX, MAGNUS. 'Experimentelle Beiträge zur Lösung der Frage über die spec. Energie der Hautnerven, I. and II. *Ztsch. f. Biologie*, 1884, XX. (II.), pp. 141–156; *idem*, III., *l. c.*, 1885, XXI. (III.), pp. 145–160. — I. contains references to the facts which seem to go against the law of specific nerve energy. Electric stimulation in different places calls forth different sensations. In any one spot, only pain or cold or warmth or perhaps touch. II. Ref. to Weber, Vierordt's and Hering's theory of warmth sensations. (Vierordt's direction or conduction.)

[1] Cf. Oscar Gellner, 'Ueber Electrolyse thierischer Gewebe' (diss., Breslau, 1870, pp. 34).

[2] Cf. Loeb, *The Dynamics of Living Matter*, Lecture V. This lecture however refers entirely to the irritability of muscular and nervous tissue.

[3] This list is not meant for a complete bibliography. It is published in the hope that a list of the writers to whom I have referred may be of use to others.

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Two Points.	More than One.	Undecided.	One Point.
15.13	8.97	0.80	75.06

Besides four out of five subjects agreed that the Vexirversuch gave clear sensations of two points. Therefore the author is of the opinion that the delusion is in part due to subjective causes.

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HENRY, CHARLES. 'Sur les relations de la sensibilité thermique avec la température.' *Comptes Rendus d. l'Académie des Science*, 1896, CXXII., pp. 1437–1439.— Exp.: hand plunged into vases with air cooled to a known temp. Minimum diff. in temp. between the two vases noted. Maximum of sensibility found at 15° C.

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KIESOW, FRIEDRICH. 'Ueber Vertheilung und Empfindlichkeit der Tastpunkte.' *Philos. Studien*, 1902, XIX., pp. 260–309.—Contains an admirable account of the literature on the anatomy of the nerve endings around the

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KIESOW, FRIEDRICH. 'Zur Psychophysiologie der Mundhöhle.' *Ztsch. f. Psychol.*, 1903, XXXIII., pp. 424-433. — Maintains against Wundt the existence of real — though sparse — touchspots on the mucous membrane of the inner cheek.

KIESOW, FRIEDRICH. 'Ueber die einfachen Reaktionszeiten der taktilen Belastungsempfindung.' *Ztsch. f. Psychol.*, 1904, XXXV., pp. 8-49. — Mainly concerns reaction time and the distinction between muscular and sensorial reactions. In general the stronger the stimulus the shorter the reactions.

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Conclusions: Pain nerves are most superficial and are least sensible to heat. Next come the cold nerves. Deepest lie the heat nerves which are the most sensitive.

(For a criticism of Thunberg cf. Paul Bader, 'Das Verhältniss der Hautempfindungen und ihrer nervosen Organe zu calor. mech. und farad. Reizen.' *Philos. Stud.*, 1903, XVIII., pp. 437–477.)

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Cf. also *l. c.*, pp. 412–414 and 591–593.

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Conductivity $= \lambda_t = A_1 e^{-bt}(1 + at)$ (p. 112).

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KORÁNYI, A. v. 'Ueber die Reizbarkeit der Froschhaut gegen Licht und Wärme.' *Centralblatt für Physiologie*, 1892-1893, VI., pp. 6-8.—Reflex caused by light on skin.

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LESSE, WERNER. 'Über den Einfluss hoher Temperaturen auf die Leistungsfähigkeit der Froschmuskeln.' Inaug. diss., Würzburg, 1893, pp. 39.—A criticism of the article by Gad and Heymans (Archiv für Anat. und Physio., Physiol. Abth., Suppl., 1890, pp. 59–115). Maintains that to Gad and Hey, mans' law "Die Erschlaffungsprozess folgt um so schneller auf den Verkürzungsprozess, je höher die Temperatur" a second law must be added, viz. Bei hohen Temperaturen erleidet daneben der Muskel, ehe er wärmestar wird eine dauernde Schädigung seiner Erregbarkeit gegen elektrischen Reize.

He confirmed Dr. Emil Heubel's result with the heart for the muscle. As Heubel found that the heart is made active again by causing blood to flow through it, so Lesse had partial success in rejuvenating the frog's muscle in the same manner.

LOEB, JACQUES. 'On the Production and Suppression of Muscular Twitchings and the Hypersensitiveness of the Skin by Electrolytes.' Reprint from Vol. X. of the Decennial Publications of the Univ. of Chicago, pp. 13.

LUDEKING, CH. 'Leitungsfähigkeit gelatinhaltiger Zinkvitriollösungen' *Annalen der Physik und Chemie* 1889, XXXIII., pp. 172–176.

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ON THE GENESIS AND DEVELOPMENT OF CONSCIOUS ATTITUDES (BEWUSTSEINSLAGEN).[1]

BY WILLIAM FREDERICK BOOK, PH.D.,
University of Montana.

Those interested in psychological research are familiar with the recent attempts at experimental investigation of the higher thought processes, and with the growing tendency toward the recognition of 'non-sensory' elements as essential components in conscious experience, which the results of these investigations have produced.

Stout (1896) was one of the first psychologists to maintain the occurrence in consciousness of imageless thought. "There is no absurdity," he says, "in supposing a mode of presentational consciousness which is not composed of visual, auditory, tactual and other experiences derived from, and in some degree resembling in quality, the sensations of the special senses; and there is no absurdity in supposing such modes of consciousness to possess a representative value or significance for thought."[2]

Mayer and Orth, in their qualitative study of association by the word method (1901), found that words were sometimes recalled by means of interpolated processes which might take the form of volitions, words, or 'peculiar conscious processes not characterizable in detail.'[3] Marbe, in his experimental study of judgment (1901), describes the same phenomenon and gives a long descriptive list of these attitudes, but declares that they cannot be adequately described. They were regularly characterized as 'peculiar, indefinite, or indescribable.'[4] Orth, in his *Gefühl und Bewustseinslage* (1903), concludes that the one characteristic common to the various imageless conscious processes was the quality of obscurity and intangibleness. In summing up Orth's conclusions Titchener says,

[1] *The Ms. for this paper was received Sept. 1, 1910 (Ed.).*

[2] *Analyt. Psych.*, I., 85 f.

[3] Titchener, *Experimental Psychology of Higher Thought Processes,* 100 f.

[4] Titchener, *op. cit.*, 101 f.

"When we try to name them, or seek to describe them, we are simply translating, substituting known for unknown; in actual experience, the attitudes are peculiar modifications of consciousness, which cannot be identified with sensation or idea or feeling."[1] Ach's observers (1905) reported that they frequently had a peculiar consciousness of what they had just before experienced. "It is as if," they say, "the whole experience were given at once, but without specific differentiation of the contents."[2] Bühler, as a result of an exhaustive study of the psychology of the thought processes (1907) concludes that, "there are thoughts without any the least demonstrable trace of any sort of imaginal groundwork."[3]

Much theoretical speculation has followed the announcement and description of these attitudes. Some have regarded them as forms of feeling, and have classed them along with Wundt's feelings of strain, relaxation, etc. Others have regarded them as a new conscious element, coördinate with sensation. A few have tried to account for them on purely physiological grounds. Others have been inclined to regard the whole contention between the 'imageless thought' advocates and the sensationalists, as a matter of individual difference in mental constitution. Some have argued that these attitudes probably represent the developed or automatized forms of imaginal processes made hazy and imageless by practice and use.[4]

The sensationalists have seemed most disturbed by the announcement and description of these imageless processes.[5] To them it has seemed as if all we have regarded as most reasonable and true in psychological interpretation was in danger of being overthrown, as if the possibility of a rational interpretation of psychic phenomena was being destroyed. They have argued that the development of the peripheral nervous system preceded the development of the central parts; that afferent

[1] Titchener, *op. cit.*, 102 f.

[2] Titchener, *op. cit.*, 103 f.

[3] Titchener, *op. cit.*, 144 f.

[4] Compare Titchener, *op. cit.*, 103–10, 180 f.

[5] Titchener, *op. cit.*, especially lecture V. Washburn, 'The Physiological Basis of Relational Processes,' *Psych. Bull.*, VI., 1909, 369 f. Angell, 'Thought and Imagery,' *Philos. Rev.*, VI., 1897, 648 f. *Ibid.*, VII., 1898, 74 f.

changes produced in the nervous system were the first kind of neural processes that occurred, and therefore (phylogenetically speaking) the source of all consciousness; that the process initiated in a sensory nerve ending, is, so far as we know, the one necessary antecedent condition of a change in consciousness; that these attitudes were so nearly sensory in nature that better observation and a more careful analysis would, doubtless, prove them wholly so.

The investigators, on the other hand, have been just as persistent in proclaiming that these 'non-sensory' processes exist. They have been steadily describing new phases and aspects of this group of phenomena. Most psychologists have become convinced not only that attitudes exist, but that they represent conscious processes which are in reality imageless.[1] All have been trying to account for these attitudes, but so far as the writer is aware no one has been able to give a satisfactory explanation (one based on controlled observation and experiment) either of their origin or nature and cause.

It is the purpose of this paper to present certain facts revealed by an experiment which the writer made some years ago, that throw important light on both these points — facts not published hitherto which reveal the nature and origin of several of these attitudes. The writer has no theory to advocate or defend and merely wishes to present the facts irrespective of any wider psychological implications which they may have.

Source of Data and Experiment.

The facts to be described were obtained from an experiment in learning typewriting, which the writer began in the psychological laboratory of Clark University, some five years ago, and published as Volume I. of the *University of Montana Studies in Psychology*, December, 1908. As may be inferred, the facts here to be presented fell outside the problem set in that experiment. They have not been written up before because of a pressure of teaching duties which prevented the writer from correlating them with the results of recent investigations of the same phenomena. It was the publication of Titchener's

[1] Washburn, *op. cit.*, 369.

book on *Experimental Psychology of the Higher Thought Processes* which brought to the writer's mind afresh the value and significance of this group of facts.

In the experiment referred to, a number of subjects,[1] including the writer (subject *X*), were given the task of learning to use a typewriter, without any directions as to methods of learning. They learned to write by both the sight method, in which they were privileged to look at the keyboard, and the touch method, when the whole keyboard was carefully screened from their view. A definite amount of practice was taken, at a stated time, each day. The main problem of the research was to determine accurately the rate of progress in learning, and to obtain from each of the learners a complete objective and psychological record of the learning.

For obtaining an introspective analysis of the learning consciousness for all levels of advancement, a plan was devised by means of which a careful cross-section analysis of the learning consciousness could be obtained from each of the learners, for all levels of advancement. In these cross-section analyses the attempt was made to carefully and systematically observe and describe all conscious processes which preceded or accompanied the making of the writing movements, or which were in any way involved in successfully carrying on the work. By making such cross-section analyses at all levels of advancement in the learning, a more or less complete psychological history of the entire process of learning was obtained. Each conscious process involved in the writing was observed, not merely at a given stage of its development, but at every stage, and the important changes in its formation and development noted and described.

This *genetic method* of observation enabled us to determine not only the specific associations and habits that were acquired in the course of the learning, it revealed exactly how they were formed and perfected. With the formation and development of the various habits acquired in the course of the learning, we are not here concerned. It was the facts revealed by the care-

[1] Compare Book, 'The Psychology of Skill,' *Univ. Mont. Pub. in Psy.*, I., 12 f.

ful descriptions, which the learners made of the conscious processes that preceded and accompanied the execution of the writing movement in the different stages of advancement, that proved important for explaining the nature and origin of the attitudes operative in the more expert stages of the writing.

But before describing these facts the general method used for obtaining and controlling these observations should be further explained. Two kinds of introspective data were gathered in our experiment. The first consisted of the observations made by the learners during the course of the regular daily tests, taken to determine their rate of progress. It was a part of the regular program to have each learner describe immediately after finishing a test such parts of his whole conscious experience as he could recall. After this he was usually questioned by the experimenter about how the work had been done on that day, and about the things which occurred to help or retard his progress. This group of data proved especially valuable for disclosing some of the factors and conditions which helped or hindered the acquisition of the special habits formed, and is irrelevant to our present purpose. The facts in these daily records which described the regular conscious processes involved in the writing were tabulated and correlated with observations made in other special practice periods arranged for making the cross-section analyses mentioned above.

These cross-section analyses were made as follows: At irregular intervals throughout the practice the learners were set to writing (copying, all writing was from copy) for the purpose of determining just how the writing at that stage was done, *i. e.*, for observing the various conscious processes which *regularly* accompanied or preceded the making of the writing movements. (The fluctuations or variations in consciousness, due to chance associations, distractions, etc., were purposely ignored in these special practices.) These special periods of practice were taken in the early stages of the learning, when changes occurred rapidly, every day, later every second, third, fifth or tenth day, as the rate of progress demanded, and were continued in each case until a complete cross-section analysis of the learning consciousness, for that stage of advancement, had

been obtained. During these special practice periods the subjects always wrote at their maximum rate, but were *privileged to stop at any time for the purpose of making* and recording observations. They were also told to stop to rest whenever they began to feel fatigued.

During these special practice periods the observers were alone in the room and made their observations unaided by the experimenter, except that they were provided with an outline giving specific directions for making an accurate and complete cross-section analysis of the learning consciousness.

The following special precautions were observed in making these cross-section analyses. As every one knows, making a thorough analysis of so complex a state of consciousness as is involved in trying to use a typewriter, is by no means an easy task. It is like looking at a forest. You can't see it all. You can observe only a small part of what is going on. Part of what you observe gets away before you can write it down. Much of what transpires cannot be adequately, or perhaps correctly expressed. There is special need, therefore, that the observations be repeated until important facts can be determined and verified.

The nature of our experiment proved especially valuable in this regard. In using a typewriter certain definite things must be done. The keys to be struck must be located (mentally and with the fingers), the several letter-making movements must be initiated and properly guided, the words to be copied must somehow be held in mind until written with the machine, etc. The special problem of the observer being to ascertain how these several tasks were performed at the level of advancement where the cross-section analysis was being made, he could take up these problems one at a time, and make repeated observations on that part of the conscious field until the facts pertaining to the solution of a problem had been clearly determined and verified. It was the regular plan to analyze the learning consciousness in this way, passing from one part or problem to another until the whole learning consciousness had been analyzed. The changes, due to practice, were not so rapid but that they remained practically the same during one of these

special practice periods. The continued writing, therefore, gave abundant opportunity for repeated observation of any conscious process involved in the writing.

Special care was also taken to guard against attaching undue importance to the various facts or phenomena observed, and to continue each special practice period until all important facts had been determined and verified. Besides the direct control which the observers had over this difficulty, objective records of the writing were obtained in all the tests, which pictured concretely on a revolving drum all that the subjects did at the machine.[1] These records were correlated with the special introspective notes and helped to check, verify and direct the observations of the learners.

The utmost care was also taken to have the observers describe only what occurred in consciousness, without any alterations or additions. The danger from this source of error is of course very great, but was guarded against by selecting experienced observers and having them keep clearly in mind the need of making a careful distinction between psychological processes and facts, and a logical report of the same. If this is kept in mind and the observations are repeated as stated above, the most complex conscious states may be accurately analyzed.

Results.

A careful examination of the data obtained from these several cross-section analyses of the learning consciousness showed that the 'mental adjustments,' 'sets of mind,' 'determining tendencies,' or conscious attitudes which were operative in the more advanced stages of the practice, represented, and, in fact, were nothing more nor less than the developed forms of the representative processes or images, operative as directing forces in the early stages of the writing. It was clearly determined that all the images which appeared in the different stages of the writing to initiate, guide, or direct the movements, were first prominent and distinct, then hazy or vague, giving way, finally, to the mental adjustments, sets of mind, determining tendencies or conscious attitudes which later initiated and con-

[1] Compare Book, *op. cit.*, 9 f.

trolled the writing movements.[1] The conscious attitudes developed by our subjects in learning to use a typewriter, therefore, represent the developed or automatized forms of certain definite representative processes, made imageless through much practice or use.

A concrete illustration will make this fact definite and clear.

In the earliest stages of touch writing the location of the letters on the keyboard was a very difficult task. Careful analysis showed that after the copy had been learned so that a word or phrase could be held in mind until written, four distinct steps were required to make each letter. There was (1) an actual spelling or thinking of the letter, (2) a process of mentally locating this letter on the keyboard, (3) the finding of this letter with the fingers, (4) the initiation and making of the final letter-making movement.

Our cross-section analyses of the learning consciousness showed that in the earliest writing of all our subjects, each of these steps was initiated and controlled by vivid imagery. Definite representative processes were required to initiate and direct each step in the work. An actual spelling or pronunciation of the letter started the process in every case — a motorization so strong in some of our subjects that it could be distinctly heard by the experimenter who sat at a table ten feet away, operating the apparatus used to take a drum record of the writing. This spelling or motorization of the letter called up in the case of observer *Y* a distinct visual image of the exact position of that letter on the keyboard. I quote from the notes: "I have to stop," he writes, "for each letter to recall by visual image where the key wanted is, then make the proper finger movements in the right direction and distance. In making each letter a distinct visual image of the letter in its exact position on the keyboard comes up to direct my fingers and hand." The process of finding a key with the fingers, when thus mentally located, was in the beginning a most laborious and difficult task, accomplished by the fingers literally feeling their way, step by step, to the position of each letter on the key-

[1] Compare Book, 'Psychology of Skill,' *op. cit.*, 42 note.

board. When the fingers had thus found the key to be struck a second spelling or motorization of the letter was required to set off the final letter-making movement.

The procedure of observer *X* was identical with the above, except that he used a different type of imagery to locate the letters on the keyboard, due to his method of learning the keyboard.[1] In his case the spelling or pronunciation of the separate letters at once called up a conscious process, regularly described as a movement of attention to the exact position of that letter on the keyboard. Careful analysis shows that this movement of attention, or idea of the position of the letter on the keyboard, was, in the earlier stages of the writing, nothing more nor less than the recall of the motor experiences — the eye, neck, and throat movements — involved in locating the keys on the visual map. I quote from his notes: "In taking up the problem of determining how I locate the letters on the keyboard again, I find that there is present, for each letter, what might be called a movement of attention to the exact position of the letters on the keyboard, following the spelling and preceding or going with the process of locating that key with the fingers. In analyzing this conscious process (movement of attention) more closely, I find that it is for the most part made up of actual and incipient eye movements in the direction of the keys, slight innervations felt in the regions of the eyes and throat, that can be clearly detected by placing the finger tips upon the closed eyes. I did not catch this process in its earliest stages of formation, but infer that this movement of attention represents the remnants of the actual eye movements involved in locating the keys with the eyes on the keyboard map, or in locating the letters on the keyboard with the eyes, in my former sight method writing."

It was this motor image or movement of attention that enabled observer *X* to locate the letters on the keyboard and start his fingers and hand in the right direction. When the fingers, thus directed, had felt their way to the proper key a second actual pronunciation of the letter was required to set off the movement for making it on the machine, as was the case with *Y*.

[1] Compare Book, 'Psychology of Skill,' *op. cit.*, 25 f.

Practice and increase of skill brought some very important changes in the conscious processes, which thus initiated and controlled the writing movements. By an elaborate process of short-circuiting, which need not here be described, direct letter associations were formed for each letter of the alphabet. These made it possible for the above-named steps to be taken as one. When letter associations became operative, getting the copy was an exceedingly easy task. The eyes were kept focused on the printed page, and the copy was gotten as written, letter by letter, and as a pure visual sensation. I quote from the notes: "The mere sight of the letters in the copy, at once sets off the movements required for making them. Visual recognition is all that is needed to initiate the proper letter-making movement." No initial or second spelling was required. The visual fixation of a letter in the copy at once called up the idea of its exact position on the keyboard, a process which came to be followed so closely, in this stage of advancement, by the hand and finger movements required for making it, that the whole process could be controlled by a single conscious span.

As these letter associations were developed and perfected an important change took place in the conscious processes involved in locating the letters on the keyboard. The notes show that the imaginal processes which formerly gave direction to the fingers and hands became, as letter associations were perfected, more and more incipient and harder and harder to observe and describe. The idea of the location of the letter on the keyboard became so imageless that the learners regularly declared it could not be described long before it disappeared from consciousness. The notes clearly show that the fingers and hands came to be guided, as to direction, by some sort of 'set of mind' which the learners felt wholly unable to analyze and describe. The notes further show that this 'mental attitude' represented the developed form of the vivid representative process used to locate the keys in the earlier stages. To illustrate: The visual image used by observer *Y* to locate the keys in the earliest writing was soon superseded by a *visual-motor* image 'which told him more quickly and surely where the key wanted was.' This in turn gave way to a process regularly character-

ized as 'a visual direction,' an imaginal process which became more and more incipient and formless until it became a mere 'set of mind' or 'mental adjustment' still necessary to give direction to the fingers and hands, but which *Y* failed to further analyze and descibe.

The 'movement of attention' used by observer *X* to locate the letters on the keybord was in the earliest writing nothing more than the recall of the motor experiences involved in the location of the letters on the keyboard map, as we have seen. This imaginal process soon gave way to a movement of attention much more incipient, but still rich in motor imagery. This was in turn superseded by a sort of 'flow of attention' in which only vague traces of motor imagery could be detected. This gave way to a 'mental adjustment' or 'attentive attitude' so incipient and imageless that it could not be adequately analyzed or described. Long before the idea of the location of the letters on the keyboard disappeared from consciousness *X* repeatedly wrote in his notes, "The process must be introspectively experienced to be understood; it cannot be adequately described."

The process of locating the keys with the fingers underwent changes in this and in the beginning of the word association stage equally interesting and important. When the four steps required to make a letter in the preceding stage had been fused into one, so that the pronunciation or visual focusing of a letter in the copy at once called up the necessary hand and finger movement for writing it, the letter-making movements could be directed and controlled as made. The fingers were guided by keeping attention carefully focused on the 'feel' (motor-tactual image) of the movement. I quote from the notes: "By far the major portion of attention goes, in this stage, to the feeling of touch and movement. These are more or less constantly watched with respect to rightness and wrongness. I find that I can avoid errors much better by attending strictly to the 'feel' of the fingers and keeping constantly in mind where my fingers are. As soon as I fail to do this my fingers go in the general direction only, and miss." In other words, the fingers were directed, in this stage, by a motor-tactual image of the letter-

making movement. In the beginning this imaginal process was very definite and distinct. The letter-making movement had to be made very slowly, and carefully attended to throughout its whole course. Every turning point in the movement had to be zealously guarded by giving close attention to its motor-tactual 'feel.' I quote from the notes: "What occurs in making a movement slowly is an attentive following of the 'feel' of the movement throughout its whole course, a vivid imaginal process which is made as fully and concrete as the time will permit." But as the practice continued this motor-tactual image became less vivid and distinct. The letter-making movement came to be directed in a more general way. The vivid motor-tactual image slowly gave way to a 'half-conscious following of the movement,' as a whole, and this to a sort of 'pre-mental adjustment for that particular movement,' a 'directing tendency' felt to be present for weeks after the learners were unable to say anything definite about it.[1]

But before this conscious process became wholly devoid of imagery for every letter a new method of locating the keys, by the fingers, became operative. As syllable and word associations began to develop, attention, which was formerly attached to individual movements for purposes of guidance, became directed to the motor-tactual image of the finger movements involved in writing syllables and words taken as a group. I quote from the notes: "A word simply means a group of movements which I attend to as a whole. I seem to get beforehand a sort of 'feel' of the whole group, which is run through with that sort of conception and direction of attention." The notes not only show that such group images were developed for the syllables and words, but that in every case the motor-tactual image for such a group of movements was at first clear and distinct, later more general and indistinct, becoming finally a mere conscious attitude which controlled the group as a whole. I quote from the notes: "There is a change in attitude, or what not, towards the keyboard that goes with the increased ability of handling it. I have noticed it for several days, but hardly know how to describe it. It is something like getting a motor-

[1] Compare Book, *op. cit.*, 33, 40.

tactual image for the whole key-board. As I write along easily and pretty fast — this is when I am most aware of this process — I seem to feel beforehand in what direction the following movements are to be made, and seem further to have a vague motor knowledge of the positions of all the keys." Later the conscious process which controlled these groups of finger movements was regularly referred to as 'getting a right orientation' or 'mental adjustment' for that group of movements, *i. e.*, it became imageless. In the matter of the conscious processes which gave direction and control to the fingers and hands, we, therefore, have the development not merely of an attitude, but of a hierarchy of attitudes.

The same is true for the mental spelling. When the syllable or word first became the unit for attention, an actual mental spelling was required to initiate and control the sequence of the movements. This mental spelling was at first characterized as an actual motorization of each letter in the word. But as practice continued and the movements got themselves linked more closely together, in the writing of words, the mental spelling became more and more incipient, persisting as an imageless conscious process long after it ceased to be consciously attended to. I quote from the notes: "Just for an instant today, when my attention was half-directed to it, I found myself incipiently pronouncing every letter as I wrote. I had felt for a long time that no more spelling was done, but find that a kind of *mental adjustment* for each letter is needed to make the movements go right." The minute character of this 'mental adjustment' was not described in the notes, but the accounts of its growing incipiency, and the many positive assertions, that such a set of mind was required to control the movements for so long a time after the spelling had seemingly disappeared, show that the former actual spelling had given way to a conscious attitude. The fact that this mental spelling slowly became linked with the conscious processes involved in locating the keys with the fingers, and that it was, in the course of the practice, superseded by a kind of group spelling, made it all the more difficult to observe and describe.

As word and phrase associations became perfected a group

spelling took the place of the former letter by letter method of spelling, above described. The letter-making movements soon became so closely linked in the repeated writing of certain phrases and words, and could soon be made so fast, that a conscious following of the individual movements became impossible, making it necessary that a higher method of controlling the sequence of the movements should be developed. The conscious processes involved in this 'group spelling' differed greatly, as to character and distinctness, with the stages of advancement. It was first characterized as 'a hurried anticipatory following of the whole course of the movements involved in writing the word or phrase,' a motor process rich in accents. This, in time, gave way to a characteristic pronunciation of the words. In describing how a certain word of two parts was written observer *Z* wrote: "I notice that I no longer spell it letter by letter, but that there are two distinct motor innervations necessary for writing the word, taking place before the first letter of each of these parts of the word is struck." As the practice continued this process of motor control became more and more incipient until it was regularly spoken of as 'getting the right mental adjustment' for a particular group of letters or words. The notes clearly show that an attitude of a higher order was developed. Here, as in the matter of locating the keys, a hierarchy of attitudes was developed. To what extent the conscious process which controlled the sequence of the movements partook of the nature of an attitude is suggested by the fact that our subjects could write a practice sentence correctly and at a maximum rate, while singing (*i. e.*, holding) the notes of a familiar tune.

A fact which gives strong support to the above statement, that certain attitudes or imageless conscious processes developed from definite representative processes, is the fact that none of these conscious attitudes was operative in the earlier stages of their development except on the good days and during the best periods of work.

In case of fatigue or when for any reason there was a slump in the learner's general physiological tone there was a regular return to an earlier level of work where the higher mode of controlling the writing gave way to a lower, the attitudes to the

more detailed representative processes which directed the writing in an earlier stage. No fact was so often described by all the observers as this slipping back from a level of control where the work was directed by conscious attitudes to a stage where a more detailed representative process was required, temporarily, to control the work. We, therefore, have in this group of facts data which proves the converse of the above proposition.

Similar illustrations might be drawn from the observations made of learning to write by the sight method.[1] Here, as in learning by the touch method, several attitudes were developed in the course of the practice. In the matter of spelling or initiating and controlling the sequence of the letter-making movements, in the location of the keys with the eyes, in the matter of developing a motor-tactual image, which came to assist the eyes in locating the keys, conscious attitudes were formed from the specific representative processes used in the earlier stages of the writing. As the practice continued conscious processes became operative that were as imageless and difficult to analyze and describe as any *Bewustseinslagen* or 'imageless thought' processes which Messer, Bühler, Marbe, Ach, *et al.*, have described.

Conclusions.

If, as there seems every reason to believe, the conscious attitudes developed by our subjects in learning to use a typewriter were of the same general nature as the attitudes, which these investigators observed and described, we may fairly conclude:

1. That the attitudes they described, like the imageless processes developed in the course of our experiments, represent nothing more nor less than the developed forms of representative processes made imageless by practice and use. Ach, in his study of *Volition and Thought*, found that awarenesses (attitudes) of 'meaning' graded off into awarenesses (attitudes) of 'relation' through intermediate forms.[2] Woodworth in his rule-of-three method of studying thought processes found that, in supplying the missing terms in his syllogisms, the transfer of

[1] See Book, *op. cit.*, 54 f.

[2] Titchener, *op. cit.*, 107.

relation from the first pair of terms to the case suggested by the third was sometimes made as the result of the recall of a definite image or word; sometimes as the result of an imageless thought process; sometimes without any consciousness at all, simply as the result of the *Aufgabe*.[1] Messer, in his *Experimental Investigation of the Psychology of Thought*, determined the various stages of development or elaboration through which a thought process might pass in consciousness, and found that his attitudes went through several distinct transitional forms.[2] C. L. Taylor[3] notes that both the imaginal representation of meaning and the attitude of 'understanding' tend to lapse as a printed text becomes familiar (241, 246). Also that an observer, who finds visual ideas essential (229) or at any rate useful (235) in the solution of a given problem, drops these ideas and employs simply 'thoughts' and attitudes in the solution of further problems of the same kind (236).[4]

These facts taken with the results of our experiments seem to the writer to warrant the inference which the above statement implies.

2. We also conclude that Stout, Woodworth, Marbe, Bühler and Orth were right in asserting the existence of conscious processes which cannot be adequately described in representative terms. That Messer and Ach were also right in maintaining that their attitudes went through a process of elaboration or development which took them through several transitional forms. Finally that Angell and Titchener were also right when they said that the attitudes they observed were not in reality imageless. We infer that all observed the same group of phenomena, but at different stages of its development. Orth, Marbe, Woodworth and Bühler caught the attitudes at the high tide of their development. Titchener and Angell caught them before they had lost all traces of their former imagery. Messer and Ach observed the same conscious processes (attitudes) at different stages of their deve opment. There is also abundant

[1] Titchener, *op. cit.*, 95 f., 152 f.

[2] Tichener, *op. cit.*, 112.

[3] 'Ueber das Verstehen von Worten und Sätzen,' *Zeits. f. Psych.*, XL., 1905. 225 ff.

[4] Titchener, *op. cit.*, 247.

evidence that some of the observers who took part in these experiments caught some of the attitudes when they had dropped almost below the threshold of consciousness.

Our results suggest that the attitudes stand mid-way between the vivid imaginal processes regularly operative in consciousness and such internal stimuli as auto-suggestions. That these subconscious stimuli occupy a place midway between the attitudes and instinctive stimuli. Conscious attitudes seem to represent a stage in a process of development which begins with vivid imaginal thought, and slowly and gradually passes downward to a stage of automatic or instinctive control.

3. Our results also show that methods for the study of psychological phenomena must be greatly refined and improved. We must not only improve our methods for making cross-section analyses of conscious processes and states, but apply, as Titchener has recently urged, the *genetic method* of observation to the study of all psychological phenomena. We have been studying conscious processes too much as if mental facts were mere static and unchangeable things. We have been so busy analyzing the tangles of special conscious states that we have failed to see important facts before our nose. We have forgotten that consciousness is a choice of processes each modified not only by what comes before and after it but that each of these processes goes through a more or less definite course of growth or development as the stream of experience widens and moves on. Every mental process should be viewed not merely as seen in cross-section analysis, it should be studied from the genetic point of view. Viewed in the light of what it was and is to be.

No one knows how many psychological caterpillars we have been calling worms; how many future mosquitoes we have been calling wiggle-tails because of our failure to do this. For ten years psychologists have been puzzled by the phenomenon represented by conscious attitudes. Many theoretical explanations have been given. The application of a carefully refined genetic method of observation to the part problems pertaining to these attitudes will tell us what they are. It is impossible to conjecture what the application of this method to the study of perception, memory, association and the higher

thought processes may not bring forth. The writer believes it will show that the abstract thoughts of the philosopher, and the psychology of faith and auto-suggestion, and belief are not such enigmas after all.

REACTIONS TO RHYTHMIC STIMULI, WITH ATTEMPT TO SYNCHRONIZE.[1]

BY KNIGHT DUNLAP.

In my paper on 'The Complication Experiment and Related Phenomena'[2] I stated my conclusion that the typical illusion of the so-called 'complication experiment' depends on a rhythmic reaction which the subject makes mechanically: that the result of this reaction (the tolerably clear vision of the pointer) seems to the subject to be synchronous with the sound or whatever discrete stimulus is used, but is really achieved previous to the said stimulus (giving negative error), or subsequent thereto (giving positive error), in all cases in which the typical error is found.

Further work, with definite sorts of rhythmic reactions seems desirable, as I have already stated,[3] and the present paper is a report of a beginning in this work.

The phenomena of the 'complication experiment' depend on an indirect attempt to synchronize reaction with stimulus. Indirect, because the subject is little, if at all, conscious through muscular or tactual sense of the reaction, but apprehends the result, *i. e.*, the visual 'picking out' of the moving pointer and attempts to make this result synchronize with the rhythmically repeated stimulus. It would be possible to arrange an experiment with a definite rhythmic reaction (*e. g.*, a finger movement) which should be controlled by the consciousness of its visual or auditory results; but it has seemed advisable to work first with attempts at direct synchronization (that is, of the stimulus with the tactual and muscular content of the reaction consciousness). So far as I can see now, either method is equally good; for the difference between what I here designate as 'direct' and 'indirect' is after all a matter of degree. Actual comparison of the two methods will however be made later.

[1] From the Psychological Laboratory of The Johns Hopkins University.

[2] PSYCHOLOGICAL REVIEW, XVII., 157-191.

[3] *Op. cit.*, 191.

There seems to have been very little work done upon the specific problem of the synchronization of a definite phase of a rhythmically repeated reaction with the stimulus. I have referred in another place to the remark of Scripture.[1] Stevens,[2] in his records of rhythmic finger movements made no measurements of the reactions except for the period after the stimulus was suspended. Stetson,[3] basing his statement on combined reactions of finger and foot, and on the stroke on a piano key, says that "a sound occurring during the beat stroke is referred to the end of the beat stroke, and becomes a part of the limiting sensation." Obviously, the conditions in the reactions mentioned are not good for the decision of the question which most interests us: How near can a reactor come to the synchronization of a definite phase of the reaction with the stimulus?

Stetson's remark, although applied to beat strokes only, suggests the possibility that in synchronizing reactions in general, the particular phase of the reaction selected for synchronization with the stimulus is of critical importance. It might make a difference whether the beginning, the end, or some intermediate phase of the movement is chosen. On this point I have made experiments on all the reactors listed below, comprising about two thousand reactions, and similar experiments on other reactors, and have found that the phase of the reaction selected makes very little, if any difference in the error, and none whatever in the direction thereof. I have also compared the results with keys of the press and release type, with similar conclusion.

Planning to use finger movement, I had a key constructed which was designed to operate with a minimal amount of noise. A lever, 7 inches in length, with pivot in the middle, bears at one end a finger button, and at the other a steel point dipping into a mercury-cup. A coil spring of adjustable tension attached midway between pivot and point draws the point down, and a

[1] Dunlap, *op. cit.*, 177. Scripture, E. W., 'Observations on Rhythmic Action,' 1899, *Yale Psychological Studies*, VIII., 103.

[2] Stevens, L. T., 'On the Time Sense,' 1886, *Mind*, O. S., XI., 391–404.

[3] Stetson, R. H., 'A New Theory of Rhythm and Discrete Succession, 1905, PSYCHOL. REV., XII., 293–350.

piece of catgut attached midway between pivot and finger-button stops the downward movement of the steel point at any desired position. There is no stop to the downward movement of the finger button. The key is mounted with a heavy metal base, so that it is not necessary to clamp it to the table. The mercury-level, spring, and catgut are finely adjustable, and the pivot is of the adjustable cone type, reducing friction to a minimum.

The key was so set that a very slight pressure on the button broke the contact between mercury and steel point. The subject was therefore instructed to make the touch of the finger-tip on the button synchronize with the stimulus.

The key, as operated, produced noise in two ways. First, from the impact of the finger on the button, which made a sound so slight as to be negligible. Second, from the checking of the lever by the catgut, when released, which was noticeable, but came so long after the chief phase of the reaction that it is hardly probable that it was a disturbing factor. An absolutely noiseless key with a satisfactory action, I have not yet succeeding in securing.

The reaction key was connected in series with the primary of an induction coil rated at three-quarter inch spark. A one microfarad condenser was connected in parallel with the break of the key. The primary current was six amperes at twelve volts. By this arrangement the break of the current, and therefore almost exactly the impact of the finger on the button, was registered by the spark from the coil, as will be further described below.

The stimulus, to which the subject reacted, was either the snap of an electric spark, passing between terminals supported about eight inches from the subject's right ear, or else the illumination of a sheet of white drawing paper by the flash of a helium tube. The sheet, fifteen by twenty inches, was supported vertically at a distance of approximately forty inches from the subject's eyes, and the helium tube was fifteen inches from and on a level with the top of the sheet. The tube was screened from the subject's eyes, so that he saw only the reflected light. By means of an ordinary double-throw double-

pole switch the spark-gap or the helium tube could be connected in the stimulus circuit at will.

The stimulus current was furnished by the secondary of a large induction coil rated at 80 millimeter spark. The primary of this coil was supplied with a current of ten amperes at fifteen volts. The interruptions of the primary current were produced by an attachment to the Schumann chronograph, described below, and the break-spark alone was used, the make-spark being shunted out of the stimulus circuit by the automatic cutout described in the REVIEW of Volume XVII., page 321.

An arm was attached to the drum-shaft of the Schumann chronograph, so that at each rotation it struck the lever of a small contact switch mounted on the frame, thus opening the primary circuit of the stimulus coil, as mentioned above. The drum was rotated by a Martin motor-rotator, the speed being reduced by belting from a small pulley on the motor shaft to a large pulley on the shaft of a fly wheel, and again from a small pulley on this fly wheel shaft to the pulley on the chronograph shaft. A heavy disc of brass was mounted on the motor shaft, adding materially to the steadiness of rotation of the system.

The motor was run on the 110-volt direct current, with lamp resistance in series and sliding rheostat resistance in parallel with the motor. This arrangement, when using a low-wound motor, not only avoids burning the brushes, but also gives more uniform speed, and more efficient speed control than can be obtained by placing the variable resistance in series with the motor.

Motor driving for rhythm apparatus cannot be said to be a great success, and would not have been adopted for this work if it had been possible to use any other system. The records, as detailed below, really show surprising constancy, but many days were wasted on which no records could be made, as the drum could not be made to run at a sufficiently uniform speed on those occasions. I expect to do further work with a pendulum apparatus.

The 250 d.v. fork was used on the chronograph, and the reaction was recorded in the ordinary fashion, by the passage of the spark from the fork-point to the drum. As the make-

spark from the recording induction coil was not of sufficient strength to penetrate the paper, no cut-out was needed in this circuit.

The gears by which the carriage screw of the chronograph is ordinarily driven were removed, and a pulley placed on the screw shaft, with belt to a motor of controllable speed. The movement of the carriage being thus entirely independent of the rotation of the drum, the carriage could be started and stopped without influencing the regularity of the rotation.

The position of the stimulus in the tuning fork record was determined as follows: The drum was so set that the switch controlling the primary circuit of the stimulus coil was on the point of opening. Then the carriage was set in motion, and the point of the tuning fork traced a line across the drum. The distance (in fork vibrations) from this stimulus line to a reaction spark measures the error in the corresponding reaction. To determine if the stimulus spark actually occurred at the moment of the opening of the switch, a number of experiments were made with the chronograph terminals connected in the stimulus circuit, so that the same spark which passed across spark-gap or helium tube also passed from fork-point to drum through the paper. The reaction record coil was of course disconnected from the chronograph for these experiments. The spark record in every case fell directly on the stimulus line. This was true even when the drum was revolved so fast that one fifth sigma was clearly readable. Only when the current was allowed to run in one direction in the primary circuit for over ten minutes was there an appreciable lag to the spark. As in the work described below the current never ran over two minutes without reversal, the practical simultaneity of stimulus with current-break is guaranteed.

Of the apparatus described, the reaction-key, condenser, spark-gap, flashing tube with screen, and *DTDP* switch which controlled spark-gap and tube, were in the reactor's room. The remainder of the apparatus was in the experimenter's room, separated from the reactor's room by another room and a hallway. Not the slightest noise from the operation of the apparatus in the experimenter's room penetrated to the subject's room. The subject's room was darkened during the experiments, except for slight leakage of light under the door.

I desired to employ stimulus rates ranging from 1/4 sec. to 2 1/4 secs. The 1/4 rate proved too fast, and even a 1/3 rate proved too fast for visual stimulation. These rates were established by counting rotations by the watch, and hence were only approximated. The actual rates were measured later by counting the fork vibrations.

The procedure in the experiment was as follows. The reactor was placed in his room and given a few minutes for adaptation. The chronograph was then set in rotation at approximately the desired rate, and allowed to run for a minute or more, the primary circuits of stimulus and record coils being broken. Then, in order, the tuning fork was set in vibration, the stimulus coil primary circuit was closed, and the record coil circuit was closed. As soon as the reactor began to receive stimulations he prepared to react, beginning his reactions as soon as he caught the rhythm. The experimenter detected the beginning of the reactions by the appearance of the spark at the point of the tuning fork. After about a dozen reactions the experimenter started the fork carriage, so that the recording commenced. After the drum was about half covered, the carriage was usually stopped, and both stimulus and record primary circuits broken, the drum remaining in rotation. After several minutes of rest, the operation was repeated, the currents now being reversed through the coils, and the remainder of the drum covered.

In some experiments on myself I covered the whole of the drum in one operation. I set the drum in rotation, and closed both primary circuits, then took my place in the reaction room and started the carriage from there by closing a switch introduced for that purpose. I had no means of knowing when the drum was filled, but kept on reacting until I was certain that I had exceeded its limits.

The experiments of which the results are given below were done in the spring of 1910, and the measurements of the records was done during the summer. Five reactors were employed; T. A. Lewis, H. M. Johnson and G. R. Wells, graduate students; J. M. Marston, an undergraduate; and myself. The collected results of the experiments are given in Tables I.–V.

Table I.

REACTOR M.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
May 18	1a	Vis.	N	1,160	0.3	26	− 66.0	37
	b			1,182	0.3	33	− 56.4	46
	2a	Aud.		1,190	3.2	29	− 63.2	19
	b			1,188	1.3	31	− 50.4	16
May 19	3	Aud.		1,032	0.1	31	− 80.4	31
	4a	Vis.		1,006	0.2	20	− 39.6	57
	b			988	0.2	27	− 43.2	29
May 20	5a	Vis.	P			25	−108.8	52
	b					24	+ 51.4	63
	c					23	+ 24.3	33
	6					23	− 3.8	33
	7a					20	− 30.6	54
	b					32	−112.7	45
	c					28	− 38.8	68
May 23	8a					22	+ 17.5	48
	b					20	−126.2	44
	c					19	−109.8	74
	9a					27	− 6.0	33
	b					31	− 32.0	28
	c					23	− 27.0	37
	10a					27	− 2.9	32
	b					27	− 17.0	38
	c					21	+ 23.0	25
May 24	11a					23	− 4.2	28
	b					26	− 19.3	28
	c					18	− 2.5	38
	12	Vis.	N	963	0.1	61	− 15.8	26
	13	Aud.		973	0.0	65	− 12.4	11
	14a	Vis.		462	0.0	19	− 50.0	19
	b			462	0.0	41	+ 70.8	29
May 27	15a	Aud.		996	0.5	34	− 41.1	27
	b			1,020	0.8	31	− 42.2	28
	16a	Vis.		1,034	0.1	33	+ 4.9	42
	b			1,038	0.1	31	− 23.1	25
	17a	Aud.		1,814	0.6	30	− 4.8	46
	b			1,792	0.2	29	+ 4.9	39
	18a	Vis.		1,822	0.7	26	+ 8.3	66
	b			1,810	0.4	29	− 9.2	30
	19a	Aud.		2,354	1.1	28	+ 36.2	64
	b			2,318	2.9	24	− 10.2	68
	20a	Vis.		2,456	3.3	27	− 29.2	51
	b			2,424	0.5	29	+ 34.4	56
May 28	21a	Aud.		2,446	1.1	26	− 42.7	65
	b			2,352	1.6	11	+ 3.6	62
	22a	Vis.		2,492	0.4	20	−110.6	118
	b			2,508	6.0	13	− 42.6	71
	23a	Aud.		1,884	8.8	22	− 98.8	52
	b			2,009	11.5	20	− 51.0	48
	24a	Aud.		1,353	12.7	24	− 74.2	36
	b			1,242	7.4	17	− 5.2	28
	25a	Vis.		1,017	1.1	33	− 50.0	29
	b			1,007	0.5	25	− 4.5	24

Table II.

REACTOR J.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
May 19	1a	Vis.	N	1,193	0.3	31	− 16.1	35
	b			1,176	0.2	31	+ 15.8	55
	2a	Aud.		1,103	2.2	23	− 11.3	29
	b			1,138	1.1	32	− 3.8	35
May 20	3a	Aud.		981	0.5	41	− 88.0	21
	b			983	0.9	38	− 55.7	26
	4a	Vis.		963	0.8	22	− 91.4	35
	b			974	0.0	18	− 33.2	30
May 23	5a	Vis.		706	0.2	49	− 13.5	44
	b			711	0.2	42	− 23.8	37
	6a	Aud.		736	0.5	42	− 28.0	7
	b			737	0.0	36	− 29.2	27
May 24	7a	Aud.		524	0.0	30	+ 6.4	11
	b			526	0.1	30	+ 20.2	11
	8a	Vis.		540	0.0	44	− 9.7	41
	b			538	0.1	30	− 39.8	18
	9a	Aud.		326	0.0	40	− 14.4	16
	b			325	0.0	27	− 22.0	9
May 25	10a	Aud.		335	0.0	47	− 12.1	14
	b			348	0.3	39	− 8.4	10
May 26	11a	Aud.		494	0.2	36	+ 18.0	8
	b			502	0.1	30	− 17.0	29
	12a	Vis.		493	0.1	43	+ 7.8	44
	b			490	0.1	30	+ 6.3	31
May 27	13a	Vis.		696	0.0	36	+ 51.4	21
	b			710	0.8	32	+ 23.0	60
	14a	Aud.		737	0.4	29	− 19.8	21
	b			709	0.9	46	+ 24.2	17
June 9	15a	Aud.		548	0.0	27	+ 22.8	9
	b			556	0.0	42	+ 8.3	16
	16a			763	0.0	39	+ 20.3	23
	b			765	0.3	26	− 14.0	15
	17a			1,433	0.2	31	+ 13.0	34
	b			1,416	1.1	29	+ 11.0	43
June 10	18a			902	1.1	40	+ 19.1	17
	b			916	0.4	36	+ 8.4	18
	19a			685	1.1	54	+ 6.0	16
	b			669	1.0	41	+ 1.6	21
	20a			408	0.1	43	+ 2.4	14
	b			414	0.0	38	+ 15.2	11
June 11	21a			454	0.1	36	+ 18.6	10
	b			455	0.0	27	+ 2.4	12
	22a			715	0.4	34	− 12.0	14
	b			705	0.0	29	− 4.6	12
	23a			1,433	0.2	31	+ 13.0	34
	b			1,416	1.1	29	+ 11.0	43
	24a		S	959	0.2	27	− 31.3	22
	b		R	962	0.0	32	− 32.4	21
	25a		R	959	0.3	28	− 44.7	13
	b		S	968	0.6	26	+ 3.0	31
	26a		S	672	0.1	29	− 7.0	21
	b		R	663	0.0	29	− 5.4	13
	27a		R	656	0.0	35	− 18.9	14
	b		S	659	0.0	32	+ 9.4	13

TABLE III.

REACTOR L.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
June 8	1a	Aud.	N	1,956	0.3	30	−46.6	47
	b			1,956	2.5	26	−45.6	63
	2a			1,253	0.0	34	− 4.1	35
	b			1,269	0.1	31	+19.6	54
	3a			720	0.2	42	+18.8	20
	b			720	0.0	33	+ 4.5	15
	4a			542	0.0	42	+31.4	19
	b			545	0.1	34	+24.6	19
	5a			342	0.0	41	+50.2	24
	b			345	0.2	36	−21.8	12
June 9	6a			352	0.1	38	+16.2	33
	b			354	0.0	—	——	—
	7a			507	0.2	37	+ 8.6	20
	b			510	0.0	35	− 8.2	13
	8a			666	0.0	47	− 8.0	31
	b			651	0.0	39	− 6.4	16
	9a			956	0.8	36	−30.8	21
	b			983	0.4	30	−15.4	25
	10a			1,524	0.2	39	−33.4	34
	b			1,576	1.1	36	−36.4	36
June 10	11a			1,474	0.8	34	−43.4	29
	b			1,494	1.2	31	+ 5.2	35
	12a			978	0.3	44	−30.7	11
	b			960	0.1	32	−22.4	23
	13a			699	0.0	40	−19.7	16
	b			699	0.0	33	+16.8	40
	14a			495	0.1	45	−62.4	16
	b			504	0.0	45	−20.0	11
	15a			327	0.0	45	−15.2	12
	b			329	0.0	36	− 3.7	9
June 11	16a			337	0.0	39	−14.8	19
	b			336	0.0	19	−12.9	10
	17a			477	0.0	28	−14.5	11
	b			477	0.0	24	− 8.2	15
	18a			628	0.0	29	−20.4	10
	b			626	0.0	27	−10.8	11
	19			974	0.6	38	−47.2	21
	20a			1,472	0.1	41	−66.9	30
	b			1,516	0.1	27	+ 4.1	16
	21a		S	1,022	0.6	26	− 4.1	31
	b		R	1,025	0.5	29	+ 6.8	37
	22a		R	1,047	0.7	27	−16.3	24
	b		S	1,037	0.6	22	−33.8	19
	23a		S	1,072	0.5	29	− 6.4	26
	b		R	1,081	0.0	27	+12.3	37
	24a		R	1,122	0.7	26	+ 7.2	25
	b		S	1,128	0.3	24	− 0.6	36

No explanation is needed for columns 1, 2 and 3 of the tables. The letters in column 4 indicate the conditions of the experiment. *N* indicates that the subject was given no specific instructions as to attention, except that he was told to make the

TABLE IV.

REACTOR W.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
June 7	1a	Aud.	N	1,261	0.4	43	−68.3	42
	b	Vis.		1,262	0.5	38	−28.0	63
	2a	Vis.		1,298	0.1	40	+ 13.2	47
	b	Aud.		1,346	0.7	40	− 47.2	50
	3a	Aud.		1,272	0.9	38	− 73.5	57
	b	Vis.		1,342	0.7	37	− 16.8	39
	4a	Vis.		1,328	0.6	38	− 41.4	36
	b	Aud.		1,372	1.5	30	− 6.7	32
June 13	5a	Aud.		717	0.2	29	−107.9	31
	b			714	0.0	24	− 81.6	21
	6a	Vis.		727	0.2	42	−172.8	38
	b			719	0.1	28	−172.0	33
	7a	Aud.		722	0.2	36	− 41.2	32
	b			718	0.1	39	− 73.6	32
	8a	Vis.		729	0.0	29	− 94.8	54
	b			725	0.2	31	− 30.4	73

touch of the finger on the key synchronize with the stimulus. Introspection on this point was not asked until late in the experiment, but indicated no conscious inequality of distribution of the attention between the stimulus and the reaction.

S indicates that the subject attempted to force the attention on the stimulus as much as possible, and *R* indicates the reverse *i. e.*, emphasis of attention on reaction.

D indicates that the attention was distracted from both stimulus and reaction. This was accomplished by having the reactor start with some number of two digits, and add to it successively the odd numbers beginning with one. This addition was commenced when the reactions commenced, and was a highly successful means of distraction.

P indicates practice. In these series the reactor was called in at the end of each group (*a*, *b*, *c*), to examine his record, and was then urged to correct the errors in succeeding series. In series marked with letters other than *P* the reactors did not see their records until entirely through with their part in the experiment. I myself was unaware of the nature of my records until after the close of the experiment, although I removed the records from the drum and varnished them, as close scrutiny was necessary in order to detect the reaction positions, and I avoided scrutinizing my records until I was entirely through.

Table V.

REACTOR D.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
May 23	1a	Aud.	N	1,004	0.0	45	−40.0	53
	b			1,016	0.0	34	− 9.6	46
	2a			684	0.0	30	−57.9	30
	b			664	0.0	28	−59.0	27
	3a			502	0.0	24	−58.0	16
	b			504	0.0	33	−38.2	14
	4a			342	0.0	36	−74.6	21
	b			340	0.0	44	−71.5	17
	5			1,106	0.0	56	+16.4	29
May 25	6	Vis.		236	0.0	—	——	—
	7	Aud.		250	0.0	87	−11.6	16
	8	Vis.		348	0.0	—	——	—
	9	Aud.		338	0.0	70	−41.8	18
	10a	Vis.		492	0.0	25	+45.6	29
	b			493	0.0	32	+ 7.1	18
	11	Aud.		507	0.2	59	−32.2	10
	12	Vis.		747	0.0	39	−20.8	25
	13	Aud.		709	0.1	46	+ 2.2	17
	14	Vis.		947	0.6	32	+48.6	34
	15	Aud.		1,114	0.1	61	−35.5	17
June 9	16	Aud.		396	0.0	70	−57.7	14
	17			853	0.1	58	+ 7.8	23
	18			1,836	0.0	72	−20.8	22
	19			712	0.1	42	−10.1	10
	20			397	0.1	53	−34.1	10
	21			713	0.0	61	−29.9	16
June 10	22		D	910	0.2	69	−56.2	20
	23			696	0.1	51	−38.0	13
	24			351	0.0	64	−52.7	15
	25			363	0.0	56	−27.6	37
	26			662	0.1	45	−53.0	17
	27			1,031	0.2	64	− 7.4	29
	28a		S	1,129	0.7	22	−32.8	34
	b		R	1,116	0.4	20	−36.8	46
	29a		R	1,098	0.3	24	−35.6	44
	b		S	1,069	0.7	19	−31.6	52
	30a		S	1,054	1.0	26	−11.4	30
	b		R	1,035	0.0	26	−28.4	36

The rates are given in *sigma*, the figures giving the number of *sigma* between successive stimulations.

The figures given in column 6 are not for the mean variation, but what is much more important, the average differences between successive rotations. The differences in any record are due to a gradual change of speed from the beginning to the end, so that although the figures given are averages, they do not differ materially from the individual differences in the same series. The actual differences between successive rotations were of course not measured directly, as I made no attempts to

get accuracy of measurement below one *sigma*. Only by comparing the first and last rotations of a given series or half-series with intermediate rotations could the variations be discovered. The formula $(a - z) \div (n - 1)$ gives the average, where a is the duration of the first rotation z of the last rotation, and n is the number of rotations. By comparing a and z with intermediate rotations the fact that the change was progressive in one direction could be established. Series with both increasing and decreasing speed are not found, probably because when the speed was near a maximum or minimum the difference between a and z was too small to be noted at all.

The mean variations are approximately 5 to 8 times the magnitude of the indicated p.v. For example, the m.v. of series 1*a*, Table I., is 1.9 sigma. The m.v. might be computed from the p.v. if it were worth while. For even values of n the formula is m.v. $= n$(p.v.) $\div 4$.

The figures in column 7 give the number of reactions in a given series, and the figures in columns 8 and 9 give the average errors and mean variations of the reactions in *sigma*. The *plus* sign in column 8 indicates that the reaction occurred after the stimulus, and the *minus* sign indicates that the reaction occurred before the stimulus with which it was supposed to synchronize.

Series 5–11, inclusive of Table I., were given at rates as near 1,000 sigma per rotation as could be established by counting rotations. It was not deemed worth while to go through the tedious process of determining the exact speeds by counting vibrations in these records.

In 6*b*, Table III., 6, 8, Table V., the rate was faster than the reactor could manage, and he reacted at a rate somewhat slower than the stimulus rate, passing thus progressively from an apparent positive error to an apparent negative error.

A visual record, corresponding to 23, Table I., was made, but accidentally destroyed before it had been measured.

The mean variation of the reactors' errors is seen to be very large throughout the records, even larger than are usually found in the complication experiment.[1] But we would expect to find

[1] Dunlap, Psychol. Rev., XVII., 160–165. Burrow, N. T., Psychol. Monographs, XI., No. 4, 32.

a larger variation in the finger reaction, because all the reactions are recorded and enter into the computation of the average error and mean variation, whereas in the complication experiment each record is of a judgment based on a number of reactions, which are themselves unrecorded. In this latter case the reactions which give conscious asynchronism may be disregarded by the reactor. In the former case they are unavoidably included in the total.

The consciously asynchronous reactions result largely from the drifting tendency which is exhibited both in the finger reactions and in the complication experiment. The reactor not only may be reacting a little before the stimulus, but he may be reacting at a rate a little faster than that of the stimulus. Each successive error in this case will be a little greater than the preceding, until a point is reached at which the discrepancy is perceived. Thus Geiger says: "Auch während der sonstigen Versuche hatten mir die Beobachter oft ungefragt erklärt, bei jeder Umdrehung scheint der Schall rückwarts zu rücken."[1] Other experimenters have found the same drifting tendency in the complication observation, but it is not always in the negative direction. In the finger reaction records I am reporting here, the drifting is strikingly apparent.

After drifting ahead or back, as the case may be, until perceptible asynchronism is reached, the reactor might make a correction by suddenly returning to a position nearer the stimulus (speaking in terms of the record), interrupting the reaction rhythm and beginning afresh, as it were. This correction actually takes place in but relatively few cases. In the majority of cases the reactor slightly retards the rate (or accelerates it, as the situation demands), and begins to drift in the other direction; but the drift is usually more irregular just after the turn, than just before. Often, after drifting to a maximum, the succeeding reactions fall scatteringly just inside the measure of the maximum.

The following series of reactions, chosen at random, are quite characteristic. The errors of successive reactions are given in sigma.

[1] Geiger, M., 'Neue Complicationsversuche,' *Philos. Studien*, XVIII., 396.

(Portion of 2*b*, Table IV.) – 8, – 30, – 48, – 134, – 112, – 126, – 178, – 128, – 178, 128, – 86, – 112, – 64, + 28, + 34, + 34, – 14, – 48.

(8*a*, Table IV.) + 10, – 26, – 76, – 74, – 76, – 60, – 10, – 24, – 90, – 112, – 158, – 128, – 176, – 192, + 48, – 258, – 212, – 160, – 98, – 42, – 98, – 42, – 144, – 156, – 72, – 42, – 84, – 72, – 124.

(First portion of 21, Table V.) – 18, – 36, – 60, – 72, – 84, – 52, – 38, – 20, + 16, – 16, – 21, – 22, – 8, + 3, 0, – 28, – 58, – 66, – 65, – 64, – 57, – 20, – 33, – 34, – 28, – 44, – 53, – 16, + 8, – 6, – 20, – 33, – 46, – 80, – 101, – 84, – 56, – 24, – 11, – 3.

Although there were in each series reactions which the reactor knew to be asynchronous with the stimulus, there was no way of excluding these reactions from the reckoning. It is impossible for the reactor to keep track of his reactions and even if it were possible, the reactions would be rendered worthless by such procedure. The experimenter has no right to cut out any reactions, even although it may be clear from the record that they are abnormal. It is quite evident that in series 8*a*, Table IV., given above, the reactions from – 176 to – 160 should be rejected. The reactor probably felt – 176 as asynchronous, and made an abrupt break in the rhythm after the next reaction (under such circumstances it is difficult to make a change until one reaction after the one felt as asynchronous), beginning with an error of + 48 and then jumping ahead to – 258, at which point the rhythm was reëstablished, but somewhat retarded for the next four reactions. If the six reactions indicated could be thrown out, the average error would be reduced about 17 per cent., but the mean variation would be reduced 50 per cent.

The mean variation is, as we might reasonably expect, independent of the average error; on the whole, the mean variation is as large where the average error is small as where there is a large average error. The mean variation is also as great, on the whole, at the faster rates as at the slower; this is rather curious, as it makes the shorter intervals relatively more irregular than the longer.

In the early experiments on the complication problem — notably those made by Wundt — it was apparently found that the error, usually negative for the slower rates, became less negative, or even positive, for the more rapid rates. Later investigations raised the suspicion that this progressive change in the error was in large part due to mere practice, the natural method being to commence with the slower rates and proceed to the faster. Gieger then attempted to separate the two factors of rate and practice, and showed clearly that for *one subject*, when the effects of practice were practically eliminated, the errors (all negative) were smaller in absolute value for the faster rates. In the cases of several other subjects he showed that practice could change the error from an original positive to a negative value. As yet we have no unexceptionable evidence that the *direction* of error is affected by rate, and we have no reason to doubt that a subject whose errors for slow rates are positive in the beginning might show a decrease in the absolute value of the error with increasing rate.

In the results of the finger reactions, as given in tables above, we find what is usually found in the complication experiment: that the magnitude and direction of the average error, although extremely variable, have no very definite relation to rate of stimulus. That under better conditions of experiment such a relation might appear is quite possible.

No very definite practice effects are discoverable in the tables, except in the case of Reactor J., who, in spite of the positive half of Series 1, is strongly negative for the first three days, becoming thereafter more positive. Reactor L. was the 'Subject III.' in Burrow's experiment[1] who made positive errors almost exclusively. Although there is a large negative average for the first record of this reactor, he shows a positive tendency in the first day's work, but becomes predominantly negative thereafter.

The oriented practice in the case of Subject M. produced rather interesting results. Immediately after the last practice series the averages are remarkably good. (Series 12 and 13, Table I.) The reactions in the first half of the following series

[1] Burrow, *op. cit.*, 31.

were felt to be anticipatory, hence the over-correction in the second half. This in itself shows an increased accuracy of observation, if not of motor performance. The increase in accuracy persisted through the third day after the end of practice, although the reactor had not attained a high degree of skill. On the following day (May 28) there was much confusion. It is evident that the limits of unnoticed asynchronism are capable of being considerably narrowed by appropriate practice.

The few series taken with emphasis of attention alternately on the stimulus and on the reaction produced results exactly similar to those produced under corresponding procedure in the complication experiment. In series 21–24 of Table III. attention to the stimulus gave a more negative error than did attention to the reaction, the signs even being opposite in three series. In series 24–27 of Table II. just the opposite effect was produced in three out of the four cases, and the same is true of series 28–30 of Table V., where the differences are less pronounced, even negligible. That changes in the direction of attention do in some cases influence the reactions is clear, and so far we have no more clue as to the mechanism by which the influence is exercised than we have to the corresponding mechanism in the complication experiment. It is quite probable that with many subjects no effect would be produced by emphasis of the attention to the one factor or the other, since the effect is frequently absent in the complication experiment. In fact, when Reactors L. and D. were tested with differential direction of attention in my experiment on the complication phenomenon, neither showed the slightest influence of this factor.

The use of two modes of stimulation was intended to furnish data, if possible, for the determination of the source of the usual delay of the simple visual reaction, as compared with the simple auditory reaction. If the delay should be due to the greater persistence of the visual sensation, as compared with the auditory, the rhythmic visual reaction might or might not show similar delay, but if the cause should be what we may loosely call motor inhibition, we may reasonably suppose that

the visual rhythmic reaction would show no more positive (or less negative) error than the auditory, since the reaction is not to the stimulus with which it synchronizes, but to the series of stimuli which precede it at relatively large intervals. In any event, the important point is to ascertain if the reactions to the two modes of stimuli do or do not show a characteristic difference.

In spite of the ill-success in the obtaining of auditory and visual series at exactly the same rates, the comparison of corresponding series in the tables above is very interesting. Bearing in mind that a relative delay in the visual reaction would give an algebraically greater (that is, more positive or less negative) error, we can conveniently compare series by noting the excess of the average error for the visual series over the average error for the auditory series.

In Table I. the comparison gives: 1 and 2, – 4.4; 3 and 4, + 39; 12 and 13, – 3.4; 15 and 16, + 32.5; 17 and 18, –0.5; 19 and 20, – 10.4; 21 and 22, – 57. In general therefore these series indicate no definite difference for the two modes of stimulation.

In Table II. we find the following differences: 1 and 2, + 7.4; 3 and 4, + 9.5; 5 and 6, + 9.9; 7 and 8, – 38 (7 being an exceptional series); 11 and 12, + 6.5; 13 and 14, + 35.

In Table IV. the differences are: 1, + 40.3; 2, + 60.4; 3, + 56.8; 4, – 34.7; average of 5 and 7 and average of 6 and 8, –41.5.

In Table V. we find: 10 and 11, + 58.5; 12 and 13, – 23; 14 and 15, + 84.1. The + 2.2 average error of series 13 should however not be allowed much weight, as every other auditory series at rate near 700 gives a negative error of from 10 to 59.

On the whole, the three reactors of tables II., IV., and V. seem to delay the visual reaction, as compared with the auditory reaction. While the delay is on the average not great it is sufficiently marked to warrant a more intensive investigation of the conditions of the modal factors in the rhythmic reaction.

The general similarity of the subjectively synchronizing

finger reaction to a rhythmic stimulus and the characteristic process in the so-called complication-experiment is pretty definitely shown by the results of these experiments. The reactions, nearly 7,000 in number, were so scattered under various conditions that no great weight of evidence has been accumulated on any of the other points raised. This wide range of the experiment in its preliminary stage was necessary in order that the next stages may be so limited as to be most effective. The problems concerning rate and mode of stimulus, practice with and without orientation, and phase of reaction-movement must be taken up in relative independence, and with perfected apparatus.

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